





AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

FOR THE YEAR MDCCCXIX.

PART I.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-BOW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
AND PRINTERS TO THE ROYAL SOCIETY.

MDCCCXIX.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

C O N T E N T S.

- I. *The Croonian Lecture. On the conversion of Pus into granulations or new Flesh. By Sir Everard Home, Bart. V. P. R. S.* p. 1
- II. *On the Laws which regulate the Absorption of polarised light by Doubly Refracting Crystals. By David Brewster, LL.D. F. R. S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S.* p. 11
- III. *Observations sur la décomposition de l'amidon à la température atmosphérique par l'action de l'air et de l'eau. Par Théodore de Saussure, Professeur de Minéralogie dans l'Académie de Genève, Correspondant de l'Institut Royal de France, &c. Communicated by Alexander Marcet, M. D. F. R. S.* p. 29
- IV. *On Corpora Lutea. By Sir Everard Home, Bart. V. P. R. S.* p. 59
- V. *Remarks on the probabilities of error in physical observations, and on the density of the earth, considered, especially with regard to the reduction of experiments on the pendulum. In a letter to Capt. Henry Kater, F. R. S. By Thomas Young, M. D. For. Sec. R. S.* p. 70
- VI. *On the anomaly in the variation of the magnetic needle as observed on ship-board. By William Scoresby, jun. Esq. Communicated by the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S.* p. 96
- VII. *On the genus Ocythoë; being an extract of a letter from Thomas Say, Esq. of Philadelphia, to Wm. Elford Leach, M. D. F. R. S.* p. 107

- VIII. *On Irregularities observed in the direction of the Compass Needles of H. M. S. Isabella and Alexander, in their late Voyage of Discovery, and caused by the attraction of the iron contained in the Ships.* By Captain Edward Sabine, of the Royal Regiment of Artillery, F. R. S. &c. p. 112
- IX. *Some observations on the formation of Mists in particular situations.* By Sir H. Davy, Bart. F. R. S. V. P. R. I. p. 123
- X. *Observations on the Dip and Variation of the Magnetic Needle, and on the Intensity of the Magnetic Force; made during the late voyage in search of a North West Passage.* By Captain Edward Sabine, of the Royal Regiment of Artillery, F. R. S. and F. L. S. 132
- XI. *On the action of crystallized surfaces upon light.* By David Brewster, LL. D. F. R. S. Lond. and Edin. In a letter addressed to the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S. p. 145
- Meteorological Journal for 1818, kept at the Apartments of the Royal Society.*

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation, for the year 1818, to ROBERT SEPPINGS, Esq. for his Papers on the Construction of Ships of War, printed in the Philosophical Transactions; and the Gold and Silver Medals on the Donation of BENJAMIN COUNT of RUMFORD, to DAVID BREWSTER, LL. D. F.R.S. for his Discoveries relating to the Polarisation of Light.

PHILOSOPHICAL TRANSACTIONS.

I. *The Croonian Lecture. On the conversion of Pus into granulations or new Flesh. By Sir Everard Home, Bart. V. P. R. S.*

Read November 5, 1818.

As this Lecture was instituted with a view to discover the principle upon which muscular motion depends, which cannot well be done till we have acquired a more correct knowledge of the structure and mode of formation of muscles; I considered that I was not greatly deviating from the direct path of this enquiry, in making the changes extravasated blood undergoes, preparatory to its being incorporated with the muscular and other structures of a living body, the subject of my last year's Lecture.

The present Lecture may be considered as a continuation of the same subject, since it is my intention to examine the changes pus undergoes in the formation of granulations or new flesh; which will be found to correspond with those that were stated to take place in the blood, so nearly, that the two fluids will be admitted to possess the same properties; and that the red colour of the globules is the principal

characteristic mark of distinction between the one and the other.

As pus, in its first formation, has the appearance of being a transparent fluid in which globules afterwards are formed, whether the transparent fluid remains on the sore, or is removed to any other surface, as was proved by experiments made in the year 1788, and since that time laid before the public in my work upon Ulcers; in this respect, pus might be considered to differ from blood; but the following observations, made by Mr. BAUER, tend to prove that a similar formation of globules is met with in the serum of blood. I shall give the remarks in his own words: "That the globules in the blood are produced in the serum, I first observed in July 1817, when I examined a small portion of human blood on a glass plate, to ascertain the real shape and size of the globules. I then found in one square of the micrometer (which was the 160,000 part of a square inch) two of these globules which were separated to a considerable distance from the rest; they were entirely disengaged from the colouring substance, and lay in pure clear serum, which covered the whole surface of the square of the micrometer. Having placed this particular square immediately under the focus of the microscope, I attentively examined the globules for about six or eight minutes, when I perceived two extremely minute opaque spots arising in the clear serum within the same square of the micrometer, and which seemed increasing in size. In a few minutes longer, I perceived five or six more ~~such~~ opaque spots arising, and gradually increasing, and assuming the same form and appearance as the two original globules; but the moisture of the serum

being nearly evaporated, I diluted it with water, when all the seven new globules, as well as the two original ones, floated in the water, and appeared of precisely the same shape and white colour; and three of the new globules were of the same size as the original ones, but the rest were smaller. When left on the glass to dry, the globules remained of the same shape and size as they were whilst floating in the serum.

“ The above experiment I have repeated a great many times with human blood, as well as with sheeps’ and calves’ blood, and the results have been always the same. When warm and fresh blood was used, the serum covering the surface of a 160,000 part of a square inch, produced from six to twelve globules, but when the serum was diluted with water, the number of globules produced was less, and they were smaller in size.

“ On the 11th of August, 1817, I poured half a pint of warm sheep’s blood into a glass vessel, and left it forty-eight hours at rest to coagulate: I then poured off the serum into another vessel, in which it remained at rest six hours; with this serum, a glass tube four inches long and three-eighths in diameter inside was filled to overflowing, and closed with a good cork, and covered with a bladder. The serum was as clear as water; and although I examined it very attentively, I could not see more than fifteen or twenty globules in the whole extent of the tube. It was kept inverted in a glass of water. At the end of seven days, upon holding the tube between my fingers, which were tolerably warm, and examining it with a double lens of considerable magnifying power, I

saw some hundreds of globules rise from the bottom, and ascend in a straight line in the centre of the tube, and when arrived within about half an inch of the upper end, they spread in all directions, and descended close to the sides of the tube; when near the bottom they re-ascended, but more rapidly than the first time, and when held longer in the warm hand, the rapidity of the motion was much increased. In two days more, I found upon examination the number of globules much greater; and on the 25th of September, 1818, the number of the globules was such as to form a sediment at the bottom of the tube of half an inch in thickness, besides a strong coat on the inside of the tube."

This experiment of Mr. Bauer's on the serum, was repeated by Mr. Faraday, at the Royal Institution, on human blood, in a tube of larger dimensions, and the serum suspended on mercury: the result was exactly the same, the number of globules was increased in ten days in the same proportion as in Mr. Bauer's experiment, and when the lower end of the tube was held in a warm hand, the same motion of the globules took place.

These experiments in proof of colourless globules forming in the serum, make this resemblance between blood and pus greater than has been generally believed.

At the conclusion of my former Lecture, I mentioned that pus, in its inspissation, has carbonic acid gas evolved, in the same manner as in the coagulation of the blood, and that I was therefore led to the opinion, that this process was the first step towards the formation of granulations; but my experiments having been made upon pus removed from the

living body, they required being repeated upon the surfaces of sores, before their results could establish the opinion I had adopted.

Before I attempted to trace the changes met with in pus upon the surface of a sore, my first object was to become more accurately acquainted with the appearance of the surface immediately under the newly secreted pus. That this surface might be examined under the most favourable circumstances for such observation, I selected an ulcer upon the leg to which no application was made but straps of adhesive plaster, and these only changed once in the twenty-four hours; and that time was chosen for the examination, which was made by a double convex lens, magnifying about eight times. Previous to the adhesive straps being taken off, the leg was laid upon a low table, so as to be immediately under the eye, and in the position in which hæmorrhage from the small vessels was least likely to take place, and obscure the surface of the sore.

That the observations might be made with greater accuracy, I requested Mr. John Griffith, one of the pupils of St. George's Hospital, to look at the sores, as well as myself, upon every occasion on which they were examined, and no change is mentioned to have taken place that was not distinctly seen by us both. A healthy sore thus examined had the following appearance: the surface was uneven, being made up of eminences and hollows. The eminences consisted of small clusters of tortuous blood vessels, the hollows were filled with pus. After remaining exposed from five to ten minutes, the following alterations were distinctly seen to take place: a very thin pellicle covered the whole surface;

this was of so transparent a nature, that a number of small bubbles of gas were seen to make their appearance in different places; in a few minutes more, horizontal canals of different sizes, filled with red blood, taking different directions, and anastomosing with one another, were seen to form. In some places, there were red points, the terminations of perpendicular canals, that had been stopped in their course, by coming against the pellicle. There were also occasional specks of extravasation, from some of the horizontal canals bursting through the pellicle.*

The changes just mentioned seemed to occur in a regular order of succession. First, the pellicle was formed on the surface. Secondly, the bubbles of gas made their appearance. Thirdly, the canals carrying red blood were observed: these, while filled with carbonic acid gas, were not to be distinguished from the semi-transparent jelly which surrounded them.

As it is difficult to describe appearances of this kind, and it is of importance that the fact of such appearances being met with, should be well established, I requested Mr. BAUER to make a drawing of a portion of the sore of which I have attempted a description, after it had been exposed for nearly sixteen minutes; and on the following day he made a drawing of the same portion, showing the progress that had been made, and that the canals formed on the first day, had on the second become permanent tubes, and had been covered over by a cuticle. These two drawings are annexed.

* If, under these circumstances, the foot was put to the ground, so weak was the covering of the canals, that it instantly gave way, and the sore was covered with blood.

It is so easy for any one to bring the facts which I have stated under his own observation, that I shall leave them to speak for themselves, but it may appear to my audience, that farther evidence is required to establish the doctrine, that they are produced entirely by the coagulation of the pus, and the extrication of the carbonic acid gas. To remove every objection which it occurred to me could be made, I put the doctrine to the test of the following experiments. Immediately after the exposure of the surface of the sore, I poured water at the temperature of 95° upon it, which washed away all the pus and although the sore was left exposed in this state ten minutes longer, none of the above mentioned appearances were produced, so that the presence of pus is necessary to their taking place.

As cold water has a power of coagulating pus more rapidly than simply exposure to the atmosphere, I applied water at the temperature of 65° , to a sore, and all the appearances were produced in so much greater a degree, that I requested Mr. BAUER would make a drawing of a portion of a sore that had been exposed for fifteen minutes under common circumstances, and at the end of that period to pour upon it water at the temperature of 65° , and, in ten minutes more, begin a second drawing of the same surface, showing to how much greater an extent the appearances had taken place; by this means proving, that the degree of coagulation was the great cause of the effects that followed. These drawings are also annexed.

As a saturated solution of sal ammoniac has a greater power of coagulating pus than any substance that I am acquainted with, (and on that account, in the year 1788, I

recommended its mixture with pus as the best criterion by which pus might be detected and distinguished from other animal fluids,) I now determined to try what effect it would have with respect to the appearance of the granulations; for although in some respects it is not a fair trial, since the chemical combination of pus with this solution might destroy the natural properties of pus, and convert it into a compound of a very different kind, still that was by no means necessarily the case.

Upon pouring a saturated solution of sal ammoniac at the temperature of 45° upon the surface of a sore, the pus almost immediately became curdled, and tortuous canals were every where seen in these masses of coagulum. There was great uniformity in the tortuous canals; they were of the same size, running first in a straight direction, terminating in a spiral turn and a half, the end of which was extremely small; they were all filled with red blood. It was remarked that, although the canals themselves were in greater number, there were fewer bubbles of gas than when the cold water had been used, more having been retained in the tubes. Some of the coagula of pus were more elevated than the general surface, and large canals filled with red blood were seen superficially passing over some of them, without any smaller ones in the immediate neighbourhood. To ascertain whether there was any vascular basis with which these canals were connected, I passed a tolerably large crooked needle under one of them, bringing out the point on the opposite side, so that the canal was distinctly seen above the flat surface of the needle: I then withdrew it, and there was not the slightest degree of extravasation of blood. This was repeated on

several different sores without any appearance of blood escaping, or the person having the slightest pain; affording a sufficient proof of the canals being formed in the coagulated pus immediately on its coagulation, before any other approximation to living animal solids had taken place.

The readiness with which the blood displaces the carbonic acid gas contained in these canals, may be explained by the great disposition the blood has to absorb this particular gas, which forms so large a proportion of its component parts.

I shall not take up the time of the Society with a farther detail of experiments, although many more were made, as the results were uniformly the same.

If I have succeeded in establishing the object of this Lecture, which is, that the coagulated pus is rendered tubular by the extrication of its carbonic acid gas, and that these tubes or canals are immediately filled with red blood, and thus connected with the general circulation, there will be little difficulty in making out the succeeding changes, by means of which the coagulated pus afterwards becomes organized; since Mr. Bauer's drawings, laid before the Society last year, trace the thin covering of the canals in the coagulated blood to the thick arterial coats met with in the testicle after the coagulum had remained a month in that situation; and it is the arteries which build up all the different structures in the body, as well in the restoration of parts, as in their original formation.

The farther prosecution of this enquiry belongs to the science of Surgery; but as the explanation which I have given of the process employed in the regeneration of parts is, I

believe, entirely different from that which is generally received, I have been desirous that an account of that process should, in the first instance, be laid before the Royal Society.

DESCRIPTION OF PLATE I.

Two-views of a small portion of a superficial sore on the leg, close to its edge, magnified 10 diameters.

Fig. 1. The appearance the surface put on after it had been exposed by the removal of the dressings for 10 minutes, none of the parts represented having been visible at the time the sore was first exposed, as it was covered with a thin core of pus. The appearances since produced are the canals carrying red blood; the red points, which are terminations of perpendicular canals; and the bubbles of carbonic acid gas. The greater part of the margin is covered with a film of inspissated pus which is become cuticle.

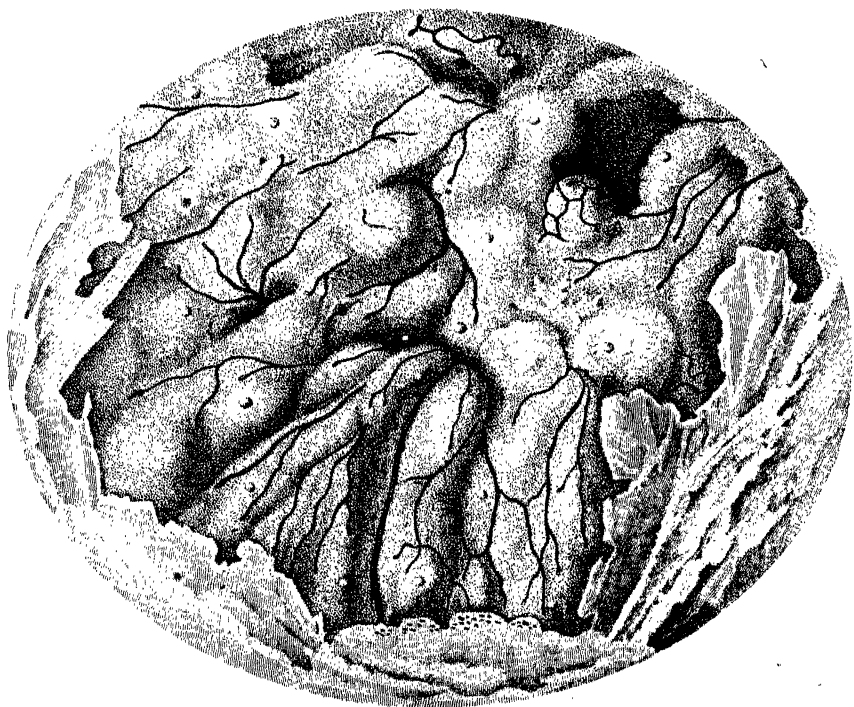
Fig. 2. The appearance the same surface put on the subsequent day at the same hour, after exposure for the same time; showing the progress on the healing process, particularly the rapids with which the sore is covered by cuticle.

DESCRIPTION OF PLATE II.

The views of a small portion of a superficial sore on the leg, magnified 20 diameters.

Fig. 3. The same as presented under exactly the same conditions as in Fig. 1. The

...in a few minutes, ...





II. *On the Laws which regulate the Absorption of polarised-light by Doubly Refracting Crystals.* By David Brewster, LL. D. F.R.S. Lon^d and Edin^b. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. G. C. B.

Read November 12, 1818.

MY DEAR SIR,

IN examining the polarising structure of acetate of copper, my attention was drawn to certain changes of colour which this crystal exhibited when exposed in different positions to polarised light. As this variation of colour was independent of the thickness of the plate, and of any analysis of the transmitted pencil, I had no hesitation in regarding it as a new affection of light, and in ascribing the phenomenon to the absorption of the homogeneous tints which formed the compound colour of the crystal. I therefore collected all the natural and artificial crystals which were characterized by any peculiarity of colour, and examined the various phenomena which they presented, when cut at different angles with the axis, and when exposed in different positions to a polarised ray. The results which I obtained during this investigation were singular and unexpected, and I am persuaded will throw considerable light on that property of transparent bodies, by which they detain and assimilate to their own substance a portion of the rays which penetrate them, while the rest are freely and copiously transmitted. As this faculty of absorbing light is related to the axes of double refraction, I

shall first describe the phenomena which are presented by crystals with One axis, and then explain the modifications which these phenomena undergo when the number of axes is increased.

SECT. I. *On the absorption of polarised light by Crystals with One axis of double refraction.*

If we fasten upon one side of a rhomboid of *colourless calcareous spar*, a circular aperture of such a magnitude that the two images of it appear distinctly separated when viewed through the spar, we shall find, by exposing it perpendicularly to common light, that the two images are perfectly colourless, and of the same intensity in every position of the rhomboid. Hence if Q be the quantity of transmitted light, we shall have the ordinary image $O = \frac{1}{2} Q$, and the extraordinary image $E = \frac{1}{2} Q$.

When the rhomboid is exposed to polarised light, the intensities of the images vary with the azimuthal angle (a) which the axis of the rhomboid forms with the plane of primitive polarisation, and may be represented by the formulæ $O = Q \cos.^2 a$; $E = Q \sin.^2 a$. But since $Q \cos.^2 a + Q \sin.^2 a = Q$ we have $O + E = Q$; that is, the sum of the intensities of the two pencils is in every position equal to the whole transmitted light, and therefore the rays which leave any one of the images by a change of azimuth, are neither reflected nor absorbed, but *pass over into the other image*. The ordinary phenomena of double refraction, consequently, afford us no reason for conjecturing that the crystals which possess this property absorb the incident light in any other way than is done by all other bodies, whether solid or fluid.

If we now take a rhomboid of certain specimens of *yellow calcareous spar*, and perform with it the experiments which have just been described, we shall obtain a series of entirely different results. The two images will now be found to differ both in colour and intensity, the extraordinary image having an orange yellow hue, while the colour of the ordinary image is a yellowish white. This difference of colour is distinctly related to the axis of the crystal, and increases with the inclination of the refracted ray to the short diagonal of the rhomb. It is a maximum in the equator, while along the axis the two images have exactly the same colour and intensity. In every position, however, the combined tints of the two images are exactly the same as the natural tint of the mineral. In comparing the intensities of the two images, the extraordinary one appears always the faintest, so that there is an interchange of rays; and while the extraordinary force carries off several of the yellow rays from the ordinary image O, the ordinary force at the same time takes to itself several of the white rays from the extraordinary image E; for if this were not the case, the extraordinary image would always have the greatest intensity, whereas, in consequence of its exchanging yellow for white light, it becomes actually fainter than the ordinary image.

If we call m and n the maximum number of rays which the extraordinary and the ordinary image interchange, and (ϕ) the inclination of the refracted ray to the axis, the intensities may be represented by the following formulæ when the crystal is exposed to common light. $O = \frac{1}{2} Q + \sin.^2 \phi m - \sin.^2 \phi n$ and $E = \frac{1}{2} Q + \sin.^2 \phi n - \sin.^2 \phi m$. The values of m and n vary in different crystals: they are always of different

colours, and in some cases they are equal to nearly one half of the transmitted light,

When the rhomboid is exposed to polarised light, a series of still more interesting phenomena is exhibited. In the position where *O* vanishes, *E* is an *orange yellow*, exactly the same as it appeared by common light; and in the position where *E* vanishes, *O* is a *yellowish white*, as before. Now it is obvious, that in the first of these positions the image *E* was not strengthened by the white light of the vanished image *O*, otherwise the image *E* would have had the same colour as $O + E$, or the natural tint of the spar; and that in the second position, the image *O* had not received the whole of the vanished image *E*, otherwise it would have had the tint expressed by $O + E$. It therefore necessarily follows, that a portion of the pencil *O* has been absorbed in the first position, and a portion of the pencil *E* in the second. The quantity of light absorbed is a maximum in the two positions where *a* is 0° and 90° , and is equal to the quantities *m* and *n*, which the two images interchange. At different angles with the axis, therefore, it is measured by $\sin.^2 \phi m$, $\sin.^2 \phi n$. When this angle is given, the absorbed light varies with the azimuthal angle *a*, and may be found from the following formula, viz. $T = O \cos.^2 a + E \sin.^2 a$, which supposes that *m* and *n* are equal to *E* and *O*. Hence when $a = 0^\circ$ $T = O$, or the whole of the pencil *E* is absorbed. When $a = 45^\circ$, $T = \frac{1}{2} O + \frac{1}{2} E$, or one half of *O* and *E* is absorbed, and when $a = 90^\circ$, $T = E$, or the whole of the pencil *O* is absorbed. When the absorbing crystal is viewed by a doubly refracting prism, the tints of the two pencils *P_e* and *P_o* will be given by the formulæ $P_e = O \cos.^2 a + E \sin.^2 a$, and $P_o = E \cos.^2 a + O \sin.^2 a$.

The property which I have now described as belonging to Calcareous spar, I have found in other twelve crystals with *One axis*. The colour of the ordinary and extraordinary images, or of the absorbed pencils *m* and *n*, is shown in the following table.

List of Absorbing Crystals with One Axis.

Names of Crystals.	Colour when its axis is in the plane of primitive polarisation.	Colour when its axis is perpendicular to that plane.
Zircon	Brownish white	A deeper Brown
Sapphire	Yellowish green	Blue
Ruby	Pale yellow	Bright pink
Emerald	Yellowish green	Bluish green
Emerald	Bluish green	Yellowish green
Beryl blue	Bluish white	Blue
Beryl green	Whitish	Bluish green
Beryl yell. green	Pale yellow	Pale green
Rock crystal, almost transparent	Whitish	Faint brown
Rock crystal yell.	Yellowish white	Yellow
Amethyst	Blue	Pink
Amethyst	Greyish white	Ruby red
Amethyst	Reddish yellow	Ruby red
Tourmaline	Greenish white	Bluish green
Rubellite	Reddish white	Faint red
Idocrase	Yellow	Green
Mellite	Yellow	Bluish white
Phosp. of lime (lilac)	Bluish	Reddish
————— olive	Bluish green	Yellowish green
Phosphate of lead	Bright green	Orange yellow
Calcareous spar	Orange yellow	Yellowish white

The property which these crystals possess of absorbing the different tints in different positions of the axis, with regard to the plane of primitive polarisation, does not belong to every specimen. There are many crystals of ruby, sapphire, emerald, &c. which give an ordinary and an extraordinary image of the same colour; and whenever this is the case, they are destitute of the property of absorbing polarised light. These two classes of phenomena are indeed invariably connected, and will ultimately be found to have the same origin.

The extreme generality of this property is indicated by the number of crystals in the preceding table, which embraces all the coloured crystals which are at present known to have only one axis of double refraction, excepting *titanite*, *molybdate of lead*, *carbonate of iron and lime*, *arsenate of copper*, certain specimens of *sulphate of nickel*, and *super-acetate of copper and lime*, in which I have not detected the property of absorbing polarised light.*

The various coloured minerals which have the cube, the regular octohedron, and the rhomboidal dodecahedron for their primitive form,† are, as might have been expected, destitute of the property of absorption; and I have not been able to discover it in differently coloured glasses, that have received the polarising structure from rapid cooling, or from mechanical compression, or dilatation.

Some of the preceding crystals, such as the *Sapphire* and the *Idocrase* exhibit different colours when common light is transmitted in directions parallel and perpendicular to their axis of double refraction. A specimen of *sapphire* had a *deep blue colour* in one direction, and a *yellowish green* in the opposite direction; and several specimens of *idocrase* had an

* See the *Phil. Trans.* for 1818, p. 211.

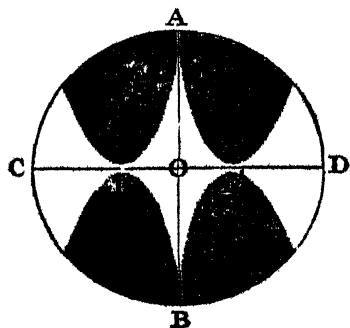
† *Id.* p. 254.

orange yellow tint along their axis, and a yellowish green tint in a direction perpendicular to their axis. The exhibition of two colours in the same mineral by common light, was first seen in *Iolite*, and the Abbé Haüy had the boldness to give it the name of *Dichroite* from this property, on the presumption that nature had limited it to this mineral. This *dichroism*, as it may be called, which, so far as I know, has never been observed in any other minerals than iolite and mica, is a very common property of crystallized bodies, as will be seen from the following section.

SECT. II. *On the absorption of polarised light by crystals with two axes of double refraction.*

The general phenomena of absorption in crystals with two axes, are nearly the same as those which have been described in the preceding section; but the quantity of light which the ordinary and extraordinary forces interchange, is regulated by new laws depending on the situation of the incident ray with respect to the two axes of double refraction.

If Oo and AB , a line perpendicular to it, are the two axes, and P, P' , the resultant axes, or the poles of no-polarisation of *Blue Topaz*: then if COD , the plane of the resultant axes is perpendicular to the plane of primitive polarisation, the polarised light incident on the plate at O will be *blue* after transmission. The blue tint preserves its intensity from O to A and B , the thickness of the plate being supposed to continue the same; but as the incident ray



passes from O to C and D, its intensity diminishes gradually, the light becoming more and more white with a slight tinge of red, till it reaches C and D, where it has the same colour as that of the topaz in common light. As the plate of topaz is turned round the polarised ray, the blue tint changes into white, according to the law given in the preceding section, excepting in the line CD, where the tint is invariably white in every azimuth. When the polarised light is transmitted along any of the resultant axes P, P', the two tints diverge from the poles in the form of a cross, as shown in the preceding figure.

The effect at O, or the interchange of the blue and white light between the ordinary and extraordinary rays, is related to the axis AB alone; and in like manner the effect at A is related solely to the other axis O. But though the axis O is more powerful in topaz than AB, yet the values of m and n for each axis do not appear to be different; a result which might have been expected from the fact, that these tints in different crystals have no relation to the intensity of their polarising forces. The diminution of the tints m and n , in passing from O to C and D, is owing to the action of the other axis O. At the points C and D blue light alone will be transmitted when AB, considered as a separate axis, is perpendicular to the plane of primitive polarisation; and at the same point white light alone will be transmitted when O considered as a separate axis of the same character is similarly placed. Hence it follows, that the transmitted light should be ~~blue, white,~~ as it actually is, the interchanged portions being as it were in a state of equilibrium.

The following table contains the tints m and n in several crystals, in which the effect will be seen in every azimuth.

List of absorbing Crystals with Two axes.

	Plane of the resultant axes in the plane of prim. polarisation.	Plane of the resultant axes perpendicular to the plane of prim. polarisation.
Topaz blue	White	Blue
—— green	White	Green
—— greyish blue	Reddish grey	Blue
pink	Pink	White
—— pink yellow	Pink	Yellow
—— yellow	Yellowish white	Orange
Sulphate of barytes		
—— yellowish		
purple	Lemon yellow	Purple
—— yellow	Lemon yellow	Yellowish white
orange		
yellow	Gamboge yellow	Yellowish white
Kyainte	White	Blue
Dichroite	Blue	Yellowish white
Cymophane	Yellowish white	Yellowish
Epidote olive green	Brown	Sap green
—— whitish gr.	Pink white	Yellowish white
Mica	Reddish brown	Reddish white

The following table shows the characters of m and n in crystals with two axes, which I have not been able to examine in every azimuth.

	Axis of prism in the plane of primitive polarisation.	Axis of prism perpendicu- lar to the plane of primi- tive polarisation.
Mica	Blood red	Pale greenish yellow
Acetate of copper	Blue	Greenish yellow
Muriate of copper*	Greenish white	Blue
Olivine	Bluish green	Greenish yellow
Sphene	Yellow	Bluish

* The tints are given in relation to the short diagonal of its rhomboidal base.

	Axis of prism in the plane of primitive polarisation.	Axis of prism perpendicular to the plane of primitive polarisation.
Nitrate of copper	Bluish white	Blue
Chromate of lead	Orange	Blood red
Staurotide	Brownish red	Yellowish white
Augite	Blood red	Bright green
Anhydrite	Bright pink	Pale yellow
Axinite	Reddish white	Yellowish white
Diallage	Brownish white	White
Sulphur	Yellow	Deeper yellow
Sulphate of strontites	Blue	Bluish white
———— cobalt	Pink	Brick red
Olivine	Brown	Brownish white

In the last eight crystals of the preceding table the tints are not given in relation to any fixed line.

The following table contains the characters of *m* and *n* in crystals, the number of whose axes I have not yet determined.

Phosphate of iron	Fine blue*	Bluish white
Actinolite	Green	Greenish white
Precious opal	Yellow	Lighter yellow
Serpentine	Dark green	Lighter green
Asbestos	Greenish	Yellowish
Blue carb. of copper	Violet blue	Greenish blue
Octohedrite	Whitish brown	Yellowish brown

Several of the preceding crystals which have a laminated structure, such as mica, epidote, &c. or such as have an imperfect transparency from a defective aggregation of their elementary crystals, frequently exhibit their absorptive qualities, and

* When the axis of the prism was in the plane of primitive polarisation.

also their system of coloured rings, by exposure to common light. The light is, in these cases, analyzed in passing obliquely through the laminæ, in the same manner as if it had been transmitted through a bundle of glass plates.

I shall now conclude this section with a particular account of some very interesting phenomena exhibited by several crystals in the preceding tables.

1. *Super-acetate of copper.* When a prism of this metallic salt is exposed to the solar rays, so that the plane of refraction is perpendicular to the axis of the rhomboidal prism, and the ray passes through the angle of the rhomboid, which is 70° , two distinct images of the sun will be observed; and the one which has suffered the greatest refraction will be *greenish yellow*, while the other will be of a *deep blue* colour. This separation of the two tints is more distinct in some prisms than in others, owing to the manner in which they are cut from the rhomboidal crystal, and in certain points of incidence the two images have the same tint. When a plate of super-acetate of copper is ground so thin as to be transparent, it has a brilliant green colour, composed of blue and greenish yellow. If it is exposed to polarised light, so that the axis of the rhomboidal prism is in the plane of primitive polarisation, the *greenish yellow* light will be entirely absorbed, and the transmitted ray will be of a *deep blue* colour. By turning the plate round the polarised ray, the greenish yellow rays will re-appear, and will gradually regain their former intensity; while the blue rays will in the same proportion be absorbed, till after one-fourth of a revolution the transmitted light will be wholly *greenish yellow*. When the faces of the plate are perpendicular to any of the two resul-

tant axes of the crystal, the blue, and the greenish yellow light have the form of a cross, the branches of which diverge from the two poles of no-polarisation:

2. *Augite*. When a plate of yellowish brown augite was exposed vertically to common light, the transmitted pencil had a moderate intensity; when it was inclined to one side, in the plane of one of its neutral axes, the light became more and more intense as the obliquity increased, notwithstanding the increase of thickness in the direction of the ray. By examining the light with a prism of calcareous spar, it was found to be all polarised in a plane perpendicular to the plane of inclination. When the plate was now inclined, from this last position in the opposite direction, the intensity of the light gradually diminished till the plate became absolutely impervious to the strong rays of the sun. The pencil which had formerly vanished when the light was analyzed by Iceland spar, now re-appeared, and gradually increased, becoming more and more *green*, while the other pencil, which became fainter, grew more and more *red*, till at a very great obliquity the one pencil became *perfectly green*, and the other a *deep blood red*. By exposure to strong polarised light, the *red* and the *green* were alternately absorbed, according to the position of the neutral axis with respect to the plane of primitive polarisation.

3. *Dichroite* or *Iolite*. This curious mineral has been long known to exhibit by common light a *deep blue* colour along the axis of the prism, and a *faint yellow* or *grey* colour in a direction perpendicular to it. I have ascertained that these two colours are those of its ordinary and extraordinary images; and even when seen by common light, that they are

related to its axes of double refraction. Dichroite has two axes of extraordinary refraction, the two resultant axes being inclined $62^{\circ} 50'$ to each other, or $31^{\circ} 25'$ to the axis of the prism. If we cut a plate of dichroite with four parallel faces, each of which is perpendicular to the resultant axes, and is inclined $31^{\circ} 25'$ to the axis of the prism, and expose it either to common or to polarised light, so as to have the plane of its resultant axes perpendicular to the plane of primitive polarisation, we shall observe the branches of blue and white light diverging in a beautiful manner from its poles P, P'. (See the figure in p. 17, which is a very imperfect representation of the phenomenon.) The white light becomes more blue from P and P' to O, and more yellow from P and P' to C and D. When the plane of the resultant axes is in the plane of primitive polarisation, the poles P, P' are marked by spots of white light, but every where else the tint is a deep blue. In the plane CADB, the mineral when seen by common light is yellow mixed with a small quantity of blue, polarised in an opposite plane. From A and B towards P and P', the yellow image becomes fainter till it changes into blue, and the weak blue image is reinforced by other blue rays till the intensity of the two blue images is nearly equal. The faint blue image increases in intensity as the incident ray approaches from C and D to P and P'. From P and P' to O one of the images is whitish, and the other deep blue, but the whiteness gradually diminishes towards O, where they are both equally blue.*

* Two crystals of Dichroite which I directed to be cut so as to exhibit these phenomena, are in the cabinet of Thomas Allan, Esq. to whose friendship I have been indebted for several of the minerals noticed in this paper.

4. *Epidote*. The cross formed at the resultant axes of epidote has its diverging branches *brown* and *sap green*, and they are distinctly seen in common as well as in polarised light. The green shades into a pale greenish white as the ray recedes from P and P' to C and D, where it is no longer divisible into two differently coloured pencils. At O the two pencils are brown and green, and at A and B they are brown and a paler green. The dichroism of the epidote is distinctly marked in common light. Along the axis of the prism, and through two of its parallel faces, its colour is a deep orange, while through the other two parallel faces it is a yellowish green.

5. *Mica*. A specimen of this mineral exhibits the branches diverging from P and P' exactly in the same manner, whether it is seen by common light, or exposed to a polarised ray with the plane of the resultant axes perpendicular to the plane of primitive polarisation. In these positions the branches PA, PB, &c. are a dark brown, and PC, PO, &c. a brownish white. When the plane of the resultant axes is in the plane of primitive polarisation, the colours of the branches are interchanged. The dichroism of mica is finely exhibited in some of its small and perfect crystals. In some of these its colour by common light is greenish yellow along the axis of the prism, and of a deep garnet colour when the light is transmitted through its faces.

6. *Anhydrite*. This mineral exhibits its dichroism by common light. When the light is transmitted in a direction parallel to the laminae, its colour is pink; but when it is incident in a direction perpendicular to the laminae, its colour is a palish yellow slightly tinged with pink.

SECT. III. *On the influence of heat in modifying the absorbent power of crystals.*

Having selected several crystals of *Brazilian topaz*, which displayed no change of colour by exposure to polarised light, I found, that after bringing them to a red heat, or even boiling them in olive oil or in mercury, they experienced such a change in their structure, as to exhibit distinctly the power of absorbing polarised light. I next took a topaz, which had one of its two pencils yellow, and the other pink. By exposing it to a red heat, the heat acted more powerfully upon the extraordinary than upon the ordinary pencil, discharging the yellow colour entirely from the one, and producing but a slight change upon the pink colour of the other.

In the formation of pink topazes by heat, it has always been supposed, that the yellow colour is actually changed into pink; but this is quite a mistake, for the pink colour must previously exist in a state of combination with the yellow, and must either form the colour or exist in the colour of one of the pencils produced by double refraction. The heat does no more than discharge one colour, and leave the other almost unimpaired. This result is of considerable practical importance to the jeweller, as it enables him to determine before hand, whether or not any topaz will receive a pink colour from heat; for if this colour exists in one of its images, which will in general be seen by exposing it to a polarised ray, he may predict with certainty the success of his experiment.

When a topaz possessing a pink tint has been taken

from the fire, it is at first perfectly colourless, and acquires the pink colour gradually in the act of cooling. By exposing it repeatedly to the action of a very intense heat, I have never been able either to remove or to modify this permanent tint.

With the view of ascertaining if the absorbing structure could be induced by heat, I exposed to a white heat several crystals of yellowish calcareous spar. After the action of the fire had been continued for some time, a sort of opalescence, or milky opacity, was induced; and the light, which went to the formation of the ordinary image, was much redder than that which formed the extraordinary ray. This effect I naturally ascribed to some change in the state of the carbonic acid; and upon continuing the action of the heat, and watching the process of decomposition, I found that when the carbonic acid was expelled from a film about the 200th part of an inch thick, its surface was covered with *vesicles arranged in straight lines parallel to the short diagonal of the rhomboid*. These vesicles had, in general, an elliptical form, with a cut or opening in the direction of their transverse axis through which the gas had escaped. After the spar was taken from the fire, a great number of the vesicles burst with a noise similar to that which accompanies the bursting of the indusium of the fern, and carried off a portion of the thin calcareous pellicle. When this pellicle was removed, the subjacent surface was covered with a series of minute parallel grooves inclined about $20^{\circ} 57'$ to the short diagonal. In repeating this experiment, and seizing the proper time for withdrawing the spar from the fire, I

have never failed to observe the fact of the arrangement and bursting of the vesicles, and I have no hesitation in concluding, that the carbonic acid is arranged in planes passing through the axis of the crystal, a result which I had formerly assumed in explaining the phenomena of double refraction. This method of studying the structure of bodies by watching them in the process of disintegration, may be found to have a very extensive application in chemical and mineralogical enquiries.

The observations contained in the preceding pages, indicate in a manner by no means equivocal, that the colouring particles of crystals, instead of being indiscriminately dispersed throughout their mass, have an arrangement related to the ordinary and extraordinary forces which they exert upon light. In some specimens the extraordinary medium is tinged with the same colouring particles, and with the same number of them as the ordinary medium : but in other specimens of the same mineral, the extraordinary medium is either tinged with a different number of particles of the same colour, or with a colouring matter entirely different from that of the ordinary medium. In certain specimens of *topaz*, the colouring matter of the one medium is more easily discharged than that of the other ; and in two specimens of *emerald*, the colouring matter which tinges the ordinary medium in the one, tinges the extraordinary medium in the other, and *vice versa*.

All those crystals in which the colouring matter of the one medium differs either in character or intensity, possess the property of absorbing the two tints according to the laws

28 Dr. BREWSTER *on the absorption of polarised light, &c.*

already mentioned, but there is also reason to believe, that polarised light suffers the same kind of absorption in those crystals whose two images have the same tint, and even in those which are perfectly colourless.

I have the honour to be, &c.

DAVID BREWSTER.

Edinburgh, Oct. 17th, 1818.

To the Right Hon. Sir Joseph Banks, Bart. G. C. B. &c. &c. &c.

III. *Observations sur la décomposition de l'amidon à la température atmosphérique par l'action de l'air et de l'eau. Par Théodore de Saussure, Professeur de Minéralogie dans l'Académie de Genève, Correspondant de l'Institut Royal de France, &c. Communicated by Alexander Marcet, M. D. F. R. S.*

Read December 17th, 1818.

§ I. L'EXAMEN des changements que les substances végétales subissent en agissant les unes sur les autres, et par leur exposition à l'action de l'air et de l'eau, est le plus sur moyen d'expliquer plusieurs effets de la végétation ; s'il ne conduit pas à ce but, il donne lieu à des expériences importantes pour la théorie de la fermentation.

L'amidon n'a presque pas été examiné sous ce rapport, ou du moins il ne l'a été que par des observations insuffisantes et indirectes ; je rappellerai celles qui me sont connues.

Lorsqu'on eut trouvé que les graines céréales formoient du sucre en germant, et que cette production n'avoit pas lieu en même temps à la température atmosphérique, dans des graines privées du contact du gaz oxygène, et imprégnées d'eau,* on fut conduit à admettre que ce gaz qui disparoit dans la germination pour former de l'acide carbonique avec le carbone de la semence, étoit le principal agent† de la conversion de la matière farineuse en sucre, sans avoir cependant aucune preuve directe de cette théorie ; d'ailleurs,

* Some experim. and observ. on the nature of sugar by W. CRUIKSHANKS.

† Ibid, and system of chemistry, by TH. THOMPSON.

l'observation que les graines céréales ne forment point de sucre avec l'eau sans le contact de l'air, n'avoit été fondée que sur leur saveur, ou sur un aperçu trop vague pour qu'il put être admis sans un examen ultérieur.

M. VOGEL a recherché l'influence de la chaleur sur l'amidon mêlé d'eau, en le soumettant à l'ébullition avec ce liquide pendant quatre jours de suite. Le mélange est devenu très fluide, il a transmis par la filtration un liquide qui après avoir été évaporé a présenté un mucilage épais et amer qui n'avoit pas le moindre gout sucré. L'amidon resté sur le filtre résistoit à l'action de l'eau bouillante, et présentoit une masse cornée très dure.*

On n'ignore pas que M. KIRCHOFF a découvert dans ces derniers temps qu'en ajoutant du gluten sec pulvérisé à une double quantité d'amidon réduit à l'état d'empois, et qu'en les faisant digérer pendant dix ou douze heures à une température de 40° à 60° R. celui-ci se convertit en partie en sucre.† Ce résultat très intéressant, mais dont les circonstances n'ont pas été suffisamment déterminées, a conduit ce chimiste à admettre que la conversion de l'amidon en sucre dans la germination, s'opère uniquement par le gluten, et à exclure l'explication de ceux qui, avant ses observations, avoient attribué ce changement à l'influence du gaz oxygène sur la matière farineuse. M. KIRCHOFF appuie sa réfutation en avançant que l'amidon seul, placé dans des circonstances favorables à la germination, ne forme point de sucre.

Les expériences que je vais décrire prouvent cependant que l'empois d'amidon seul et abandonné à lui-même forme au

* Annales de Chimie, T. 82.

† Beitrage zur chemie und physiq. vom SCHWEIGER, 14 Band.

bout d'un certain temps une quantité considérable de sucre cristallisable et qui a beaucoup de rapport avec celui que ce chimiste a obtenu par l'acide sulfurique. Cette décomposition spontanée de l'amidon mêlé d'eau fournit encore d'autres produits, et en particulier une espèce de gomme et une matière intermédiaire entre celle-ci et l'amidon. Leur proportion varie suivant plusieurs circonstances qu'il est très difficile de déterminer. Pour indiquer l'ordre de mes recherches et le procédé de mes analyses, j'exposerai en détail la première que j'ai faite à ce sujet. C'est celle dans laquelle la décomposition de l'amidon a été la plus avancée, quoique ce ne soit pas l'opération qui m'ait fourni la plus grande quantité de sucre.

§ II. J'ai placé de l'empois formé avec vingt grammes d'amidon,* et douze fois son poids d'eau distillée bouillante dans un vase cylindrique où le mélange offroit une grande surface à l'air, et fermoit une couche de deux centimètres d'épaisseur. Ce vase recouvert d'un grand récipient sous le quel l'air extérieur pénétrait facilement, a été laissé en repos pendant deux ans dans un lieu où la température s'élevoit en

* L'amidon employé à cette expérience étoit de l'amidon de froment, pris dans le commerce sous le nom d'amidon de *première qualité*. On n'en pouvoit pas séparer une quantité notable de gluten : car après avoir mis cet amidon en macération pendant dix jours avec trente fois son poids de vinaigre distillé, il a fourni par la filtration une liqueur qui étant évaporée a laissé un résidu acide, mol, dont le poids n'étoit que la quatre millième partie de l'amidon employé ; et ce résidu retenoit un peu d'eau, le mucilage du vinaigre, et environ la sixième de son poids d'acide acétique. L'eau froide mise en macération pendant vingt quatre heures sur cet amidon, ne lui enlevoit pas une quantité sensible de son poids. 100 parties de cet amidon séché à 18° R. perdoient par le dessèchement à la température de l'eau bouillante 13,66 de leur poids. J'ai varié mes expériences avec des amidons pris dans différents magasins ; ils m'ont tous fourni des résultats analogues à ceux que je décris dans le texte.

été jusqu'à 18° R. Lorsqu'à cette époque, le mélange a été retiré, il a présenté une pâte grise, liquide, couverte de moisissure, et presque sans odeur ; elle ne changeoit alors, ni dans aucune époque antérieure de son altération, ni après avoir été délayée et filtrée, les couleurs végétales employées comme réactifs ; elle ne pouvoit plus faire les fonctions de colle. Le poids de l'amidon avant son altération étoit beaucoup plus grand que celui de cet amidon altéré ; le premier étoit au second comme 100 : 76,2, après leur dessèchement à la température atmosphérique, et comme 100 : 180,46, après leur dessèchement à la température de l'eau bouillante.

§ III. La pâte dont je viens de parler a été mise en macération pendant vingt quatre heures avec une quantité d'eau *froide* à peu près égale à vingt fois le poids de l'amidon considéré dans l'état sec, puis jetée sur un filtre de papier, et lavée avec une nouvelle dose de liquide ; elle a fourni une liqueur transparente, d'un jaune pâle, qui a laissé après son dessèchement un extrait un peu mol, et dont le poids étoit égal à la $\frac{47}{100}$ ème soit environ à la moitié de celui de l'amidon qui avoit formé l'empois. Cet extrait a été dissous dans une fois son poids d'eau, puis mêlé avec dix fois son poids d'esprit de vin à 35° de l'aréomètre de BAUME : il a dissout le sucre en précipitant l'espèce de gomme que je décris dans la note A à la fin de ce mémoire. Cette gomme retenoit alors un peu de sucre et d'un produit intermédiaire entre la gomme et l'amidon ; on l'a purifiée en la traitant par l'alcool comme l'extrait précédent, ~~en la~~ dissolvant ensuite dans l'eau, et en la filtrant. Le poids de cette gomme desséchée, transparente, et soluble dans l'eau froide en toute proportion, étoit égal à deux grammes, ou à la dixième de l'amidon mis en expérience.

§ IV. Les dissolutions alcooliques évaporées d'abord à un feu doux et ensuite à la température atmosphérique ont commencé par présenter un résidu sucré, transparent, en consistance de miel, qui retenoit encore un peu de gomme que j'ai comprise dans le produit précédent ; elle a été séparée par une nouvelle dissolution dans l'eau, et la précipitation par l'alcool. Il s'est formé au bout de peu de jours dans le résidu de l'évaporation de la liqueur alcoolique, des cristaux le plus souvent réunis en groupes sphériques, hérissés de lames transparentes ; ils présentoient au microscope lorsqu'ils étoient isolés, des lames quarrées et des cubes. Bientôt ce résidu s'est presque entièrement converti en une masse opaque de sucre concret, jaunâtre, doué de l'odeur propre à la cassonade : il est resté long-temps gluant par son mélange avec un syrop plus difficile à cristalliser ; mais par une longue exposition à l'air, le tout a paru sec et homogène. Son poids étoit égal à la 0,37^{ème}, soit à plus du tiers de l'amidon employé pour cette expérience.

Ce sucre que je n'ai pas dépouillé de son principe colorant passe à la fermentation alcoolique avec une très petite quantité de levure ; si elle étoit trop abondante ou si elle montoit à $\frac{1}{2}$ ^{ème} du sucre, ce mode de décomposition n'auroit point lieu.

100 parties d'alcool absolu bouillant en dissolvent 5 ou 6 parties. L'alcool à 35° BAUME' en dissout $\frac{1}{8}$ ^{ème} de son poids à une température de 20° R. si le sucre est dans l'état sec ou entièrement concrifié, car s'il étoit un peu visqueux par son mélange avec une matière plus difficile à cristalliser, cet alcool en dissoudroit une plus grande quantité.

Ce sucre s'est liquéfié à la température de l'eau bouillante ; il a perdu par cette opération entre la 0,07 et la 0,08 de son

poids. Comme ses principales propriétés conviennent au sucre d'amidon préparé par l'acide sulfurique, il est très probable que ces deux substances sont identiques.

§ V. Après avoir extrait par l'eau froide, les produits gommeux et sucrés contenus dans le résidu de la décomposition de l'amidon, je l'ai soumis deux fois pendant une ou deux minutes à l'ébullition avec une quantité d'eau égale à celle qui avoit été employée dans l'opération précédente. Les décoctions filtrées après leur *réfroidissement* ont fourni par l'évaporation à siccité, un résidu fragile, jaune, à demi-transparent ; son poids étoit égal à la septième partie de l'amidon employé. Cette substance sur laquelle je reviens plus en détail dans la note B à la fin de ce mémoire, a des propriétés intermédiaires entre le principe gommeux précédent et l'amidon : elle se dissout en petite quantité dans l'eau froid, et en toute proportion dans l'eau bouillante, en formant avec l'une et l'autre, des dissolutions transparentes, non gélatineuses, et qui se filtrent facilement au travers du papier. J'ai donné ici, pour éviter les périphrases, le nom de *amidine* à ce produit, soit aux modifications de l'amidon dans lesquelles il acquiert la propriété de se dissoudre dans l'eau froide, en conservant la faculté de colorer en bleu la solution aqueuse de iode.

§ VI. La pâte qui est restée sur le filtre après l'action de l'eau bouillante, sur l'empois altéré, pouvoit dès lors faire les fonctions de colle ; elle étoit presque noire, et dans l'état sec son poids étoit égal à la sixième partie de l'amidon mis en expérience. L'éther ou l'alcool absolu mis en digestion sur ce résidu, y a dissout en partie une matière colorante, brune, qui en étoit précipitée par l'eau. Cette matière colorante desséchée s'est présentée sous l'apparence d'une huile ou d'une

résine épaisse et visqueuse, mais trop peu abondante pour que j'aie pu la mieux examiner. Elle n'équivaloit qu' à trois millièmes de l'amidon employé.

§ VII. On sait qu' une partie d'amidon se dissout en peu de minutes à l'aide d'une douce chaleur, et sans décomposition apparente, dans quarante fois son poids d'acide sulfurique délayé ou composé d'une partie d'acide sur douze d'eau : mais cette liqueur soumise à l'ébullition dans les mêmes proportions avec le résidu amilacé pulvérisé, sur lequel l'eau bouillante et l'alcool n'avoient plus d'action, n'en a pu dissoudre que la 0,35^e partie, soit environ le tiers. La partie dissoute qu'on pouvoit précipiter en partie par l'alcool,* étoit de l'amidon non décomposé mêlé d'une petite quantité d'amidine.

§ VIII. La matière insoluble par l'acide sulfurique délayé se montrait après son dessèchement, sous l'apparence de grumeaux opaques, très fragiles ; ils se sont dissous facilement (sauf un petit résidu composé de ligneux et de charbon)

* L'amidon forme avec l'acide sulfurique une combinaison qui cristallise en aiguilles transparentes, prismatiques, très fines ou très allongées. Pour obtenir ce produit, on précipite par de l'alcool la dissolution d'amidon dans l'acide sulfurique délayé, on lave avec de l'alcool le précipité, qui est un mélange d'eau, d'acide sulfurique, d'amidon pur et de la susdite combinaison : elle se dissout en partie par une petite quantité d'eau froide. Cette dissolution filtrée fournit par une évaporation lente et spontanée les cristaux dont j'ai parlé, qui sont mêlés avec de l'acide sulfurique libre, qu'on enlève par leur lavage avec de l'alcool. Ces cristaux sont en partie décomposés par l'eau qui en précipite de l'amidon ; mais en filtrant la dissolution aqueuse, en évaporant, et en enlevant avec de l'alcool, l'acide sulfurique mis à nud, on obtient de nouveau la combinaison ou sèche et cristallisée d'acide sulfurique et d'amidon. Ce dernier aussi précipité de cette combinaison par l'eau, a subi une légère altération, car il ne peut précipiter qu'en rouge de vin la solution aqueus l'iode.

à l'aide d'une douce chaleur, dans dix fois leur poids d'une lessive de potasse qui contenoit un douzième de cet alcali ; Ils ont formé ainsi une solution brune, très liquide, qui n'avoit point la consistance visqueuse et gélatineuse propre aux solutions alcalines d'amidon, et ils en étoient précipités par l'acide sulfurique délayé, sous la forme d'une poudre combustible, jaune, légère, qui après son dessèchement offroit une masse noire, brillante semblable à du jayet. Cette dernière délayée dans l'eau, coloroit encore en bleu la solution aqueuse de iode.

La substance végétale dont cette matière amilacée insoluble à chaud par l'acide sulfurique délayé, se rapproche le plus, est le ligneux ou le bois : elle en diffère cependant en ce qu'elle est soluble dans des lessives de potasse plus étendues que celles qui peuvent dissoudre ce dernier, et en ce qu'elle colore en bleu la solution aqueuse d'iode. Je désignerai cette matière sous le nom de *ligneux amilacé*.

§ IX. Le charbon mêlé de ligneux que la lessive alcaline précédente n'a pu dissoudre équivaloit à $\frac{1}{25}$ ème de l'amidon employé. Ils n'ont laissé après leur combustion, qu'une très petite quantité de cendres.

§ X. Pour reconnoître le genre d'altération que l'air avoit éprouvé pendant la formation de tous les produits précédents, j'ai exposé sous des récipients pleins d'air et fermés par du mercure, de l'empois récent d'amidon, et d'autre part, de l'empois à différentes époques de son altération par l'action antérieure de l'air. Dans toutes ces expériences, le volume de l'air renfermé dans les récipients n'a subi aucun changement ; le gaz oxygène en a été en partie détruit, mais il a été remplacé par un volume égal de gaz acide carbonique. L'altération que l'amidon a fait subir ainsi à l'air ne s'est

opérée que lentement ; dans le cas où elle étoit la plus rapide, et où j'ai employé de l'empois récent, quinze grammes de cette substance contenant $\frac{1}{12}$ ème d'amidon n'ont formé sous une grande surface * pendant deux mois à environ 18° R. que cinquante centimètres cubes de gaz acide carbonique dans un décimètre cube d'air. La même quantité d'empois après deux ans d'exposition à l'air libre, produisoit dans une expérience semblable, un volume de gaz acide égal au quart du précédent. Ces expériences montrent que l'influence du gaz oxygène sur l'amidon se borne à lui enlever du carbone. J'ai vu de plus, en même temps, que la perte de poids que l'amidon altéré éprouve après son dessèchement est beaucoup plus grande que celle qui résulte de la soustraction de ce carbone. On peut en conclure que l'amidon en s'altérant à l'air, perd sous forme d'eau une grande proportion de son oxygène et de son hydrogène. Le carbone enlevé à l'amidon par l'air étoit à l'eau qui se formoit en même temps dans le rapport de 1 : 74, pendant les deux premiers mois de l'altération.

§ XI. La colle d'amidon laissée en repos à l'air libre se couvre de moisissure, et il étoit possible que les résultats que j'ai obtenus fussent l'effet de cette végétation ; j'ai empêché son développement, soit en agitant l'empois tous les jours, soit en le plaçant dans de grandes jares fermées, pleines d'air, qui avoit été exposées à la température de l'eau bouillante, immédiatement avant l'introduction de l'amidon, et je n'en ai pas moins obtenu tous les produits dont j'ai parlé précédem-

* Lorsque l'empois n'offre pas une grande surface à l'air, il dégage du gaz acide carbonique à la formation duquel cet air n'a aucune part, et alors on n'observe plus d'égalité entre les volumes du gaz oxygène consumé et du gaz acide carbonique produit.

ment, savoir, du sucre, de la gomme, de l'amidine, une substance d'apparence huileuse, une matière ligneuse, de l'eau, du charbon, et enfin du gaz acide carbonique dont l'oxygène appartient à l'air ambiant.

§ XII. En répétant les expériences précédentes, soit avec de l'amidon de froment, soit avec celui de pomme de terre, dans différentes circonstances, et toujours avec le contact de l'air, j'ai obtenu les mêmes produits ; leur proportions seules ont varié. Elles m'ont paru indiquer, 1° qu'il se formoit moins d'eau lorsque l'empois offroit moins de surface à l'air ; 2° qu'une température un peu plus élevée que celle qui avoit été employée dans ma première expérience favorisoit beaucoup la production du sucre ; 3° que tout celui qui s'étoit produit dans cette épreuve n'avoit pas été recueilli parce qu'il s'étoit détruit par une fermentation trop prolongée.

§ XIII. Pour déterminer d'une manière précise si l'empois d'amidon se décompose sans le contact de l'air en formant du sucre, j'ai rempli, à la réserve de 8 centimètres cubes, une bouteille avec 300 centim. cubes d'empois, § II; Il a été préparé dans ce vase qui a été bouché et mastiqué avec du ciment pendant que l'eau étoit chaude, pour expulser l'air contenu dans le petit espace désigné plus haut. Cette bouteille a été placée en été dans une chambre où la température s'est maintenue entre le 18° et le 20° R.

J'ai exposé en contact avec l'air dans un vase ouvert, très évasé, à côté du précédent, du même empois qui a été agité tous les jours avec une spatule, et auquel on a ajouté successivement de l'eau distillée pour remplacer celle qui s'évaporait. Il ne s'y est point formé de moisissure, et il est devenu en peu de temps tout à fait liquide.

Au bout de 38 jours, j'ai analysé l'empois altéré, contenu dans les deux vases. Il avoit perdu l'odeur propre à l'amidon récent, et n'en avoit point contracté de bien marquée. Il s'étoit dégagé dans la bouteille fermée un air condensé qui s'est échappé à son ouverture avec une sorte d'explosion, et que j'ai trouvé par des expériences subséquentes être du gaz acide carbonique mêlé de gaz hydrogène.

Après avoir réduit par l'agitation les deux empois en une consistance uniforme, et les avoir pesés, j'en ai séparé une partie déterminée pour la sécher et juger ainsi du changement de poids que l'amidon avoit subi. Ils m'ont fourni des résultats très différents.

L'amidon avoit diminué de poids par la fermentation *avec* le contact de l'air dans le rapport de 100.83 avec des dessèchements opérés à la température de l'eau bouillante.

L'amidon alérié *sans* le contact de l'air n'avoit subi, après son dessèchement au même degré, aucune diminution de poids; il paroissoit même avoir augmenté de $\frac{1}{500}$ ème. Cette augmentation se confond avec les erreurs d'observation; mais si l'on considère que pendant la fermentation, il a formé du gaz acide carbonique dont je n'ai pas tenu compte, et qu'il a diminué de poids, en se décomposant et en produisant de l'eau pendant un dessèchement* qui a duré deux ou trois

* Les dessèchements avant et après la fermentation, ne sont pas faits dans des circonstances égales. Le premier s'opère sur une matière déjà sèche en apparence et inaltérable à l'air par l'eau qu'elle récite. Le second est fait sur une substance réduite en pâte et très altérable à l'air dans l'eau qui l'environne. J'ai préparé de l'empois avec 100 parties d'amidon séché à 80° R.; cette pâte réduite par l'évaporation à l'état sec sous la température précédente, n'a représenté par son poids que 98,5 parties d'amidon. Ce résultat indique qu'une substance analogue, dont le poids dans l'état sec ne se seroit point trouvé changé après un pareil traitement, auroit

blables, c'est-à-dire à une températ. de 18° à 20° R. : J'ai mastiqué seulement à la bouteille où se faisoit la fermentation sans le contact de l'air, un tube recourbé pour recueillir sur le mercure le gaz qui se dégageoit. 30 grammes de cet amidon réduit à l'état d'empois avec 360 grammes d'eau, ont dégagé, dans 42 jours, 26 centimét. cubes de fluide aëriiforme qui étoit composé en volume de 80 parties de gaz hydrogène presque pur (voyez la note D, à la fin de ce mémoire), et de 16 parties de gaz acide carbonique. Quoique ce dernier paroisse le moins abondant, il n'est pas douteux qu'il n'ait été produit en quantité environ quatre fois plus grande que le gaz hydrogène, parce que le volume du liquide qui étoit presque quatre fois plus grand que le gaz dégagé, a retenu le gaz acide carbonique dont je n'ai pas tenu compte, et a émis au contraire presque tout le gaz hydrogène.

Par la fermentation *en contact* avec l'air, l'amidon de pomme de terre a diminué de poids en raison de 100 : 77,7 avec des dessèchements à la température atmosphérique, et en raison de 100 : 85,8 par des dessèchements à la température de l'eau bouillante.

Le poids de l'amidon altéré *sans* le contact de l'air, avec des dessèchements à la température de l'eau bouillante, étoit précisément égal au poids du même amidon avant sa fermentation, en ne tenant pas compte de la perte de poids qu'il a subie par le dégagement du gaz acide carbonique, ni de celle qu'il a éprouvée après cette fermentation par sa décomposition au contact de l'air pendant le dessèchement. L'amidon a paru avoir diminué de poids dans le rapport de 100 : 94 avec des dessèchements à 18° R. ; mais ce changement n'étoit

dû qu'à la différente faculté hygrométrique de cette substance avant et après son altération. Dans ce dernier état, 100 d'amidon perdoient 10,6 d'eau à 80° R. tandis que cette perte montoit à 16,41 avant la fermentation.

100 d'amidon de pomme de terre séché à 18' R. ont laissé par la fermentation spontanée pendant 42 heures, sans le contact de l'air, un résidu qui a fourni après son dessèchement à la même température,

Sucre	-	-	-	35,4.
Gomme	-	-	-	17,5.
Amidine	-	-	-	18,7
Ligneux amilacé	-	-	-	7
Ligneux mêlé de charbon				
quantité imponderable.	-	-	-	
Amidon non décomposé	-			9,4.
				<hr/> 88.
Perte dans l'analyse				6.
				<hr/> 94.

100 d'amidon de pomme de terre ont fourni par leur fermentation avec le contact de l'air, et dans des circonstances d'ailleurs égales,

Sucre	-	-	-	30,4.
Gomme	-	-	-	17,2.
Amidine	-	-	-	17.
Ligneux amilacé	-	-	-	4,4.
Ligneux mêlé de charbon	-			0,2.
Amidon non décomposé	-			9,3.
				<hr/> 78,5.

Les principaux résultats que je déduis de ces expériences

sont ; 1°, que l'air n'a aucune influence sur la formation du sucre dans la décomposition spontanée de l'amidon ; 2°, que la fermentation sans le contact de l'air diffère de celle qui s'opère avec ce contact, en ce que dans cette dernière, l'amidon perd sous forme d'eau une grande proportion de son oxygène et de son hydrogène ; tandis que dans la fermentation sans le contact de l'air, l'amidon bien loin de perdre de l'eau paroît s'approprier au contraire une petite quantité des éléments de ce liquide.*

§ XIV. Pour comparer la fermentation spontanée de l'amidon avec le procédé par lequel M. KIRCHOFF a produit du sucre dans l'espace de dix ou douze heures en mêlant du gluten sec pulvérisé avec une double quantité d'amidon qu'on réduit à l'état d'empois, et en les exposant à une température de 40° à 60° R., j'ai divisé ce mélange en trois parties égales qui ont été chauffées (avec le degré et le temps prescrits) au même bain marie, dans trois vases différents : Le premier d'entr'eux étoit plein et exactement fermé ; le second étoit ouvert, évasé, et en libre contact avec l'air ; le troisième étoit un ballon fermé plein d'air, dont l'empois n'occupoit que la cinquantième partie.

Le poids du produit de ces opérations, même de celles qui avoient été faites avec le contact de l'air, étoit égal aux poids du gluten et de l'amidon avant leur mélange, ou du moins il ne leur étoit inférieur que d'une quatre millième, en faisant les dessèchements à la température de l'eau bouillante.

Dix grammes d'amidon ont produit dans le ballon fermé

* Le sucre obtenu dans les expériences où la fermentation de l'amidon de froment et de pomme de terre n'a duré que cinq ou six semaines, n'a point pu cristalliser. Il n'en étoit pas de même lorsque les fermentations ont été beaucoup plus prolongées.

et plein d'air, 50 centimètres cubes de gaz acide carbonique à la formation duquel cet air n'avoit eu aucune part, car après son mélange avec la potasse, il contenoit la même proportion de gaz oxygène qu'avant l'expérience.

Il s'est formé dans les trois vases à très peu près la même quantité de sucre, c'est-à-dire environ la septième partie de l'amidon employé. Les petites différences entre les résultats, et qui pouvoient être accidentelles, se trouvoient en faveur de l'exclusion de l'air pour la production du sucre. La matière sucrée bien purifiée et obtenue par les procédés du gluten diffère du sucre qu'on obtient de l'amidon fermenté sans mélange étranger,

1°, En étant beaucoup moins soluble dans l'alcool aqueux. 100 parties de cette liqueur à 35° de l'aréomètre de BAUME n'en ont pu dissoudre que 2,85 parties à une température de 18° R.

2°, En ce qu'elle forme avec dix fois son poids d'eau une dissolution où la décoction de noix de gale produit un précipité blanc, très abondant, et dont on ne voit aucun vestige avec les dissolutions des autres sucres.

La gomme obtenue par le procédé décrit § 3, équivaloit à la quinzième partie de l'amidon employé ; elle différoit de celle de l'amidon fermenté, 1°. en formant une dissolution aqueuse qui, en opposition avec le résultat de M. KIRCHOFF, étoit abondamment troublée par la décoction de noix de gale ; 2°, en colorant en bleu la solution aqueuse de iode qui y manifestoit ainsi la présence de l'amidine ou de l'amidon.

Après avoir extrait par l'eau froide les principes gommeux et sucrés, j'ai essayé inutilement de séparer par l'eau bouillante, l'amidine du résidu de l'opération ; il se réduisoit par

ce traitement en une colle qui ne transmettoit rien par les filtres. La matière inattaquable par l'eau froide se dissolvoit en partie dans l'acide sulfurique délayé. Le résidu indissous étoit un mélange de gluten et de ligneux amilacé. Ce dernier se séparoit dans la liqueur par sa différente densité; il ne donnoit par sa blancheur aucun indice de matière charbonneuse, et il se dissolvoit en entier dans des lessives de potasse très étendues. Quoique l'on reconnut dans ces produits tous ceux de la fermentation de l'amidon seul, ils étoient impossibles à extraire dans leur état de pureté à cause de l'intervention de la matière glutineuse qui les faisoit adhérer les uns aux autres.

Il se forme dans cette opération, suivant l'observation de M. KIRCHOFF, un acide que la liqueur retient après l'ébullition. Ce produit est dû au gluten qui le dégage par sa fermentation, sans le secours de l'amidon. L'absence de cet acide dans la fermentation de l'amidon pur, montre que la formation du sucre par les procédés antérieurs n'a pas été due à la présence accidentelle du gluten.

Malgré ces différences, la décomposition spontanée de l'amidon seul sans le contact de l'air, et celle qui s'opère par l'intermède du gluten, ont dans leurs résultats généraux plusieurs caractères semblables très frappants. Il y a production de matière sucrée, de gomme, de ligneux amilacé, et probablement d'amidine. Il y a dégagement de gaz acide carbonique. Il n'y a point de précipitation de charbon et point d'eau formée ou de perte de poids dans le produit sec de l'opération. On peut en conclure que le gluten en s'unissant à l'amidon ne fait qu'accélérer une fermentation que ce dernier auroit subie par lui-même sans cette influence, qui modifie légèrement les produits de l'opération.

§ XV. M. KIRCHOFF a trouvé* que quelques acides différents de l'acide sulfurique convertissoient l'amidon en sucre. Pour connoître si le gaz acide carbonique qui se forme par la fermentation de l'amidon avoit été la cause de la production du sucre dans mes expériences, j'ai introduit dans un grand ballon de verre, de l'empois d'amidon de froment, § 11, dont il n'occupoit que la cinquantième partie, et après y avoir fait le vide par la pompe pneumatique, je l'ai rempli avec du gaz acide carbonique pur. Ce ballon, fermé par un robinet, a été exposé pendant quarante jours dans un lieu où la température s'est maintenue entre le 18° et le 20° R. L'amidon n'a presque pas été décomposé, car 100 parties ont fourni par cette opération,

Sucre	-	-	-	1.
Gomme	-	-	-	0,36.
Amidine mélé d'amidon	-			1.

Les $\frac{1}{1000}$ ème de l'amidon ne paroissent avoir subi aucune altération. Le gaz acide carbonique met donc obstacle à la fermentation de l'amidon, et à la formation de tous les produits dont j'ai parlé.

Résumé des principales observations contenues dans ce Mémoire.

L'amidon réduit par l'eau à l'état d'empois, et abandonné à sa décomposition spontanée, à une température entre 16° et 20° R., produit soit avec le contact de l'air, soit sans cette influence,

1°, une espèce de sucre semblable à celle qu'on obtient de

* Journal de Physique par DE LA METHERIE, T. 74.

la même fécule par l'intervention de l'acide sulfurique delayé et d'une plus haute température.

2°, une espèce de gomme qui a un grand rapport avec le principe gommeux de l'amidon torréfié.

3°, une matière que j'ai désignée sous le nom d'amidine, et dont les propriétés sont intermédiaires entre celles de l'amidon et de la gomme précédente.

4°, une substance qui s'approche du ligneux par son insolubilité dans l'eau bouillante et dans plusieurs acides ; mais elle tient de la nature amilacée en colorant en pourpre la solution aqueuse d'iode.

La décomposition spontanée de l'amidon fournit encore d'autres produits ; mais leur présence et le mode de leur formation sont subordonnés à l'action ou à l'absence de l'air atmosphérique pendant la fermentation.

Lorsque cette décomposition se fait *avec* le contact de l'air, l'amidon produit une grande quantité d'eau, dans laquelle le gaz oxygène atmosphérique n'entre point comme principe constituant. Il se forme du gaz acide carbonique dont l'oxygène appartient à l'air atmosphérique. L'amidon dépose encore dans cette circonstance du charbon qu'on ne sépare qu'imparfaitement, et qui rembrunit tous les produits de l'opération. Le gaz oxygène n'est point absorbé dans cette fermentation qu'en tant qu'il forme le gaz acide carbonique dont je viens de parler. Le poids du résidu sec de la décomposition de l'amidon avec le contact de l'air pèse moins que l'amidon employé. La soustraction dû carbone par l'air n'entre que très peu dans ce déchet qui est du presque uniquement à l'eau formée par l'amidon, et qui se volatilise.

Lorsque la décomposition spontanée s'opère *sans* le contact

de l'air, l'amidon ne produit point d'eau, il dégage une petite quantité de gaz acide carbonique et du gaz hydrogène pur ou presque pur. Il ne dépose point de charbon. Le poids du résidu de cette fermentation après le dessèchement à la température de l'eau bouillante s'est trouvé dans mes expériences égal au poids de l'amidon employé à la même température : mais comme je n'ai tenu compte ni de la perte qu'il a subie par le dégagement du gaz acide carbonique, ni de celle qu'il a éprouvée par sa décomposition dans un long dessèchement avec le contact de l'air, il me paroît probable que l'amidon dans sa fermentation sans ce contact, fixe ou s'approprie en petite quantité les éléments de l'eau.

Mes expériences sans l'influence de l'air n'ont été ni assez prolongées ni assez multipliées, pour indiquer si sa présence augmente la quantité du sucre ; leurs résultats à cet égard ont varié. Il est probable que l'air la diminue, en détruisant tous les produits de l'opération.

La conversion de l'amidon en sucre par l'intervention du gluten, dans l'espace de quelques heures, et par une température élevée, fournit des produits sucrés et gommeux qui diffèrent des substances obtenues dans l'opération précédente, en ce qu'ils donnent avec l'eau, des dissolutions où la décoction de noix de gale indique la présence de la matière glutineuse par des précipités abondants. Ce principe donne au produit sucré d'autres propriétés distinctives très saillantes. Il s'engendre de plus dans l'empois mêlé de gluten, un acide qui ne se manifeste point dans la fermentation de l'amidon seul, et qui paroît dû exclusivement à la fermentation du gluten. D'ailleurs la décomposition spontanée de l'amidon

sans le contact de l'air, et celle qui s'opère par l'intermède de la matière glutineuse, ont en général des caractères semblables. Le gluten en s'unissant à l'amidon, ne paroît qu'accélérer une décomposition que celui-ci auroit subie plus tard, sans cette influence.

FOURCROY à désigné quelques opérations chimiques dans lesquelles il se produit du sucre, sous le nom de fermentation saccharine. Il avoit principalement fondé cette distinction sur le goût sucré que prennent plusieurs fruits par la coccion, et sur la formation du sucre dans l'acte même de la végétation et de l'animalisation : mais le premier résultat, celui de la saveur, étoit trop indéterminé ; et le second ne s'adaptoit pas au nom de fermentation, qui suppose l'acte d'un mouvement spontané et intestin dans des substances végétales ou animales désorganisées et privées de vie. Aussi cette désignation n'a-t-elle pas été adoptée. Mais puisque nous voyons par des effets précis que la formation du sucre a lieu dans le sens le plus strict attaché au mot de fermentation, il convient de distinguer cette dernière, et de la faire précéder toutes les autres, en lui conservant le nom de fermentation saccharine.

Genève ce 7 Octobre, 1818.

Note A, sur la gomme produite par la fermentation spontanée de l'amidon, § III. et § XIII.

Cette gomme purifiée par sa dissolution dans l'eau, sa filtration, et sa précipitation par l'alcool, est, après son desséchement, transparente et presque sans couleur, si la fermentation

s'est faite sans le contact de l'air ; mais si cette opération a été très prolongée avec ce contact, si elle a été accompagnée de moisissure, la gomme est jaune et un peu trop molle pour pouvoir être pulvérisée. 100 parties de cette gomme à 15° R. perdoient par ce dessèchement à la température de l'eau bouillante 11,75 d'eau ; dans cet état, elle est toujours très friable. Elle n'attire pas d'ailleurs l'humidité de l'air ; elle y est inaltérable : mais sa dissolution aqueuse s'y décompose au bout d'un certain temps, sans passer à l'état acide, en prenant une odeur putride, et en déposant d'épaisses mucosités.

Elle est insoluble dans l'alcool, et elle est soluble dans l'eau en toute proportion. Deux parties de ce liquide et une de gomme offrent une solution très fluide, mais elle devient filante et visqueuse lorsque le poids de la gomme excède celui de l'eau.

La dissolution d'une partie de gomme dans dix d'eau n'est troublée ni par l'acetate de plomb, ni par le sous acetate de plomb, ni par la décoction de noix de gale, ni par la liqueur des cailloux.

Elle n'altère pas la couleur de l'infusion de tourne-sol.

Elle ne produit aucun changement de couleur dans la solution aqueuse d'iode.

Elle est faiblement troublée par l'eau de baryte.

Elle ne produit point d'acide muqueux avec l'acide nitrique.

Cette gomme a beaucoup de rapports avec celle qu'on obtient de l'amidon torréfié. Elles diffèrent seulement en ce que l'eau de baryte fait un précipité beaucoup moins abondant dans la solution de gomme d'amidon fermenté ; en ce que cette dernière a un peu de flexibilité à une basse température,

et en ce qu'elle a une couleur beaucoup moins foncée, et presque nulle, si la fermentation s'est faite sans le contact de l'air.

Note B, sur la matière intermédiaire entre la gomme et l'amidon, ou sur l'amidine, § V. et § 13.

Le résidu de la décomposition spontanée de l'amidon, après avoir été traité par l'eau froide, abandonné à l'eau bouillante un principe que ce liquide, après son refroidissement et sa filtration, retient en dissolution, et que j'ai désigné sous le nom d'amidine. On la purifie dans l'état sec, en la lavant avec une petite quantité d'eau froide, en la faisant dissoudre dans l'eau bouillante, et en filtrant la dissolution après son refroidissement. Ce produit ne me paroissant être qu'une modification spéciale de l'amidon, ne conservera pas le nom que je lui ai donné ici pour abrégé : car on ne surchargera pas la science de nouveaux noms pour les modifications infinies que peut présenter toute substance végétale ou animale par une légère altération.

L'amidine obtenue par l'évaporation de sa dissolution aqueuse, se présente, suivant le mode du desséchement, en fragments blancs, opaques, et irréguliers, ou sous l'apparence d'une matière jaune pâle, à demi transparente (comme de la gomme arabique), et très friable. Elle est insoluble dans l'alcool. L'eau froide mise en macération sur l'amidine en dissout environ $\frac{1}{10}$ ème de son poids, et présente après sa filtration une liqueur sans couleur, et très fluide.

L'eau dissout l'amidine en toute proportion à une température d'environ 50° R., et elle en retient en dissolution, après

son refroidissement, une beaucoup plus grande proportion que celle dont se charge ce liquide lorsqu'il agit à froid sur cette substance. La décoction peut être rapprochée par l'évaporation au point de contenir le quart de son poids d'amidine en dissolution, sans se troubler ou sans se convertir en pâte et en gelée par le refroidissement ; ce qui n'a point lieu pour l'amidon. Lorsque la dissolution d'amidine est plus rapprochée, elle se précipite en partie, par le refroidissement, en une matière blanche et opaque ; mais cette dernière se dissout, en présentant une liqueur transparente, à une température de 50° R. : sous ce rapport elle se rapproche de l'inuline. La dissolution aqueuse d'amidine faite à froid, et qui en contient la dixième de son poids, se colore en bleu avec la solution aqueuse d'iode, et présente avec ce réactif tous les effets de l'amidon.*

La même dissolution est coagulée en une pâte blanche et opaque par le sous-acétate de plomb : l'acétate neutre n'y fait qu'un précipité peu sensible.

Elle est abondamment troublée par l'eau de baryte, et point par l'eau de chaux. La décoction de noix de gale n'y produit pas de changement bien marqué.

Les solutions aqueuses de potasse dissolvent l'amidine. Ces combinaisons sont très fluides, et ne se présentent point dans l'état visqueux et filant de celles d'amidon. Les acides foibles en précipitent l'amidine avec toutes ses propriétés.

L'alcool y produit aussi un précipité abondant, mais ce dernier retient une certaine proportion d'alcali qui fait que

* Annales de Chimie, T. 90. Mémoire sur l'iode par MM. COLIN et GAULTIER de Claubry.

l'amidine précipité ne se colore en bleu par l'iode, que lorsqu'on y ajoute un acide.

L'amidine diffère donc principalement de l'amidon, en ce que l'eau froide peut la dissoudre, en ce qu'elle ne forme point de gelée avec l'eau bouillante, ni des combinaisons visqueuses avec les lessives de potasse. Les caractères qui la distinguent du principe gommeux dont j'ai parlé précédemment, sont, 1^o, de n'être pas soluble dans l'eau froide en toute proportion; 2^o, de colorer en bleu la solution aqueuse d'iode; 3^o, de former avec l'eau une dissolution qui est coagulée par le sous-acétate de plomb.

Je n'ai point pu obtenir d'amidine par la germination du froment. Si ce produit s'y forme, c'est sans doute en trop petite quantité pour qu'on puisse le séparer de l'amidon. Dans l'analyse que j'ai faite de 100 parties de froment, avant, et après sa germination, 6 parties de sa substance farineuse ont paru détruites par cette végétation, et elles ont été remplacées par $3\frac{1}{2}$ parties de mucilage, et $2\frac{1}{2}$ de sucre sec déliquescent. Si, comme on peut le présumer, l'amidine se forme en aussi petite quantité que ces produits, elle peut rester confondue avec l'amidon sans qu'on puisse l'en séparer. D'ailleurs je n'ai pas trouvé, malgré les assertions contraires de MM. PROUST et DÖBEREINER, que l'amidon du froment fut notablement changé dans ses propriétés par la germination.

Note C, sur le changement de poids qu'éprouve l'amidon par sa fermentation à l'air, § II. § X. § XIII.

En analysant le sucre produit par l'action de l'acide sulfurique et de l'eau sur l'amidon (Bibliothèque Brit. Sc. et Arts. v. 56) j'ai trouvé que cette production s'opère par la fixation

des éléments de l'eau dans l'amidon.* Je suis parvenu à la même explication, on plutôt à sa confirmation en montrant que la quantité de sucre formée par ce procédé est plus grande que celle de l'amidon employé, lorsqu'ils sont séchés l'un et l'autre à la température de l'eau bouillante. Cette observation pourroit paroître opposée à celle qui montre que le résidu de la décomposition de l'amidon à l'air pèse moins que l'amidon employé : mais il est peut être superflu d'observer que ces effets ne sauroient être comparés ; parce que dans celui de l'acide sulfurique, le sucre est en dernier résultat le seul produit notable de l'opération ; tandis que dans la fermentation de l'amidon à l'air, il se forme plusieurs produits dans lesquels les éléments de l'eau sont répartis d'une manière très inégale, et se perdent en partie par l'évaporation.

Par la fixation des éléments de l'eau dans l'amidon pour former le sucre, je n'entends pas qu'ils s'y trouvent dans l'état d'eau solidifiée ou d'eau de cristallisation ; la manière dont ils sont répartis est encore indéterminée.

Cette fixation de l'eau a lieu sans doute plus souvent qu'on ne le pense dans le traitement des substances végétales ou animales par les procédés ordinaires de nos laboratoires. J'ai trouvé que les nouvelles propriétés que les graisses acquièrent par la saponification, tiennent principalement à la fixation des éléments de l'eau dans la graisse.

100 parties de graisse de porc lavée et filtrée, m'ont fourni par leur combustion dans du gaz oxygène,*

* Cette analyse ne s'accorde pas avec celle que BERNARD a faite de la même substance en la traitant par l'oxyde de cuivre. Ses résultats ainsi qu'un grand nombre de ceux obtenus par ce procédé, m'ont paru pécher par excès d'hydrogène. (Annales de Ch. et de Phys. T. V.)

Carbone	-	2	78,848.
Hydrogène	-	-	12,182.
Oxigène	-	-	8,502.
Azote	-	-	0,473.

100

100 parties de la même graisse saponifiée ou précipitée de son savon de potasse par l'acide muriatique contiennent,

Carbone	-	-	75,747.
Hydrogène	-	-	11,615.
Oxigène	-	-	12,325.
Azote	-	-	0,313.

100

Le gaz oxigène consumé et le gaz acide carbonique produit par la combustion de la graisse, avant et après sa saponification, sont presque dans le même rapport, savoir comme 100 : 71,5 dans la 1^{ère} analyse; et comme 100 : 72,7 dans la 2^{de}. Il en résulte qu'on pourroit presque considérer ces deux espèces de graisse comme ayant la même base unie a différentes quantités d'eau, quoique ce ne soit point là probablement le véritable mode de leur composition.

Les expériences de CHEVREUL que j'ai vérifiées ne sont pas opposées à cette explication en indiquant qu'il y a augmentation de poids dans le produit de l'opération. 100 parties de graisse de porc fournissent suivant ce chimiste, 94 ou 95 de graisse saponifiée, et environ 9 de principe doux sans compter la perte qui a lieu dans la manipulation : ces produits paroissent donc surpasser le poids de la graisse qui les a fournis.

Note D, sur le gaz hydrogène produit par la fermentation de l'amidon, § XIII.

On peut être surpris que le gaz hydrogène formé par la décomposition spontanée de l'empois d'amidon, soit du gaz hydrogène pur, et non point un gaz hydrogène carburé semblable à celui des marais : mais toutes les substances végétales que j'ai fait fermenter par un procédé analogue à celui qui a été employé pour l'empois, ont dégagé du gaz hydrogène pur ou presque pur, abstraction faite du gaz acide carbonique qui y étoit mêlé, et de la faculté qu'avoient quelques unes d'entr'elles de ne dégager que ce dernier.

2,85 grammes de gluten frais qui contenoit la 0,37 partie de son poids d'eau, et au quel j'ai ajouté 16 grammes de ce liquide, ont dégagé dans cinq semaines, après avoir été placés sous un récipient renversé plein de mercure, 80 centim. cubes de gaz, sans y comprendre celui que l'eau a retenu. Les 80 étoient composés de 60 de gaz acide carbonique et de 20 de gaz hydrogène qui n'a pas formé une quantité notable de gaz acide carbonique par sa combustion.

Le froment dans une fermentation semblable n'a dégagé que du gaz acide carbonique, sans mélange de gaz hydrogène.

3 grammes de graines de pois sèches aux quelles j'ai ajouté 12 grammes d'eau, ont dégagé dans trois semaines, par un procédé semblable, 117 centimètres cubes de gaz qui contenoit 88 de gaz acide carbonique et 29 de gaz hydrogène.

J'ai répété ces expériences sur une livre de pois, en les enfermant sous un récipient plein d'eau, renversé sur ce liquide en contact avec l'air. La pesanteur spécifique du gaz

hydrogène dégagé, abstraction faite du gaz azote qui le souilloit, étoit 0,088g. 100 parties de cet air inflammable que j'ai pu analyser plus en grand que les précédents, ont consumé 55,45 de gaz oxigène, en formant 2,64 de gaz acide carbonique.

Ces expériences ont été faites avec quelques autres graines ; et celles qui ont produit du gaz hydrogène l'ont toujours dégagé pur ou presque pur, mais mêlé avec du gaz acide carbonique dans une proportion qui n'a souffert que peu de variations, c'est-à-dire environ dans le rapport de 1 : 4 en y comprenant le gaz acide que l'eau a retenu ; tel étoit aussi le mélange de gaz que l'amidon et le gluten ont fourni.

La formation d'un gaz hydrogène aussi pur par la fermentation n'avoit pas été observée : elle conduit à chercher la raison pour la quelle la fermentation des marais produit un gaz inflammable aussi différent du précédent.

IV. *On Corpora Lutea.* By Sir Everard Home, Bart. V. P. R. S.

Read January 14, 1819.

IN May, 1817, I laid before the Society an account of the human ovum, and not only showed the cavity of the corpus luteum from which it had escaped, but another corpus luteum in the same ovarium, which had made a considerable advance in its growth.

Ever since that time I have been actively employed, with the assistance of Mr. BAUER, in tracing the rise and progress of the corpus luteum to its full growth, its use, and afterwards its decay. The result of our labours is contained in the present communication, and in the drawings which accompany it.

Corpora lutea are never met with before puberty; the natural structure of the ovarium is therefore more readily ascertained before that period. It is nearly the same in different animals; is of a loose open texture, in which, more particularly near the circumference, a number of small cells of a globular form are met with. This structure is shown in the annexed drawings, in the human ovarium, [Plate III. fig. 2.] in that of the cow, [Plate VI. fig. 2.] and in that of the sow, [Plate VIII. fig. 2.]

The corpus luteum, from its first appearance, seems to be an entirely new substance, distinct from that of the ovarium itself; it is never formed within the cells, but in the substance of the ovarium; [See Plate IV. fig. 2 and 4: Plate VI. fig. 4: and Plate VIII. fig. 4:] and compresses the surrounding

parts so much, that when full grown, and even when there are several in the same ovarium, that body is not much increased beyond its natural size.

The structure of the corpus luteum is of a very particular kind, and is not distinctly seen in small animals, or in those that have numerous litters; but in the cow, which has commonly only one calf at a birth, the corpus luteum is so large, that when it is magnified, the structure can be made out; it is a mass of thin convolutions, bearing a greater resemblance to those of the brain than of any other organ. Its form is an irregular oval with a central cavity, and in some animals its substance is of a bright orange colour when first exposed; all these appearances are most accurately displayed in the annexed drawings of it in the cow. [Plate VII. fig. 2, 3.] Corpora lutea are found to make their appearance in the ovaria at the age of puberty, and continue to succeed each other as the young are produced, till the period arrives when breeding no longer goes on. [Plate III. fig. 7; Plate IV. fig. 6; Plate IX. fig. 6.]

As the object of the present paper is to draw conclusions from the appearances that are represented in the annexed drawings, in proof of corpora lutea being the structures in which the ova are formed; of their being produced previous to, and independent of sexual intercourse; and when they have fulfilled their office of forming ova, being afterwards removed by absorption, whether the ova are impregnated or not; I shall not take up the time of the Society longer than in detailing a number of facts, which indeed will be doing little more than giving a catalogue raisonnée of Mr. BAUER's drawings, which put these facts upon record. That corpora

lutea are formed in a state of virginity, is proved both in the human species and the hog tribe, as will be seen in the annexed drawings. In a young woman of twenty years of age with a perfect hymen, one of the ovaria was found by Mr. BAUER to contain a corpus luteum, in the cavity of which there was an ovum which had nearly arrived at its full size; the second covering or chorion had already formed, by means of which the ovum had a slight adhesion to the inner surface of the corpus luteum. [Plate III. fig. 4.] When this ovum was examined after it had been removed from its situation, [Plate III. fig. 5,] its figure was the same as that found in the uterus and described in my former paper; it only differed in being smaller in the proportion of $\frac{15}{200}$ to $\frac{18}{200}$, in the whole being transparent, and in the chorion not having extended itself completely over the anterior surface of the ovum. The Fallopian tube on that side was fuller than the opposite. The fimbriæ were spread out, and unusually vascular; so that every preparation was made for the reception of the ovum into the tube. No sexual intercourse had taken place. [Plate III. fig. 3.]

I have met with *corpora lutea* in virgins at 14; and know of two instances of girls having children still earlier, one at 13, the other at 12.

Sir JOHN SEBRIGHT, whose knowledge respecting pigeons is well known, informs me, that when mated, they lay eggs earlier than when kept from the male; they do not lay a greater number of eggs, but they lay them at all seasons, while the others lay only in the spring.

In the cow, the age of puberty is considered to be two

years old, and the corpus luteum represented in the engraving is that of a first calf. [Plate VI. fig. 4; and Plate VII. fig. 2, and 3.] In the hog tribe, the age of puberty is six months; and in the series of drawings of their ovaria, one is given from a sow pig at four months, in which no corpora lutea had begun to appear. [Plate VIII. fig. 2.] In another, between five and six months, several have made a considerable advance, [Plate VIII. fig. 4.] and in one nearly six months they were completely formed, and their cavities filled with blood, similar to the human corpus luteum containing the ovum, but no ova were detected. [Plate VIII. fig. 6.]

In another virgin pig, nearly six months old, Mr. BAUER was so fortunate as to meet with corpora lutea in the very act of bursting to part with their ova: the appearance is shown in Plate IX. fig. 1 and 2. From this most fortunate occurrence we learn that animals part with their eggs whether there is sexual intercourse or not, and this is done with such force, that the cavity of the corpus luteum is absolutely inverted, so that the ovum is exposed completely to the emission of the male. The extravasation of blood in rupturing the ovarium, and inverting the corpus luteum, is in many instances so great, that some of it passes out through the vagina, which when met with, is considered a sign of impregnation having taken place. As soon as the ovum is expelled, the corpus luteum recovers itself, and returns to its natural state. When the ovum accidentally adheres by a portion of the chorion to the inner surface of the corpus luteum, as is shown to be the case in Plate III. fig. 4, it may retain its hold; and however complete its expulsion, may be carried back, and form what is

called an ovarium case, the foetus growing in the ovarium; several cases of this kind are met with, particularly one now in the collection of the Emperor of RUSSIA, purchased from the late Mr. CRUIKSHANK, teacher of Anatomy in London.

An instance of corpora lutea forming in succession, and probably to the same number, whether the female is ever impregnated or not, is shown in a drawing of the ovarium of a woman who died a virgin at 47 years old; the fragments of seven corpora lutea are distinctly seen in it, and in the opposite there were five, putting on the same appearance. [Plate III. fig. 7.]

After the escape of the ova, the corpora lutea have their cavities distended with blood, which coagulates, loses its colour, and forms a white solid mass surrounded by broken portions of the corpus luteum. [Plate IV. fig. 4 and 5.] These become smaller and smaller, till they disappear. A series of them is shown in the human ovarium, six weeks and nine months after impregnation; also after a woman had borne 12 children, and had for many years left off breeding. [Plate V. fig. 6.]

The remains of the corpus luteum, at nine months after impregnation of the ovum, are so indistinct as hardly to be recognized; but in the opposite ovarium there is commonly a corpus luteum far advanced, forming another ovum; and it will be found that all the preparations of corpora lutea taken from the ovaria of women who die in child-bed, actually belong to this new ovum not yet completely formed. [Plate V. fig. 2, and fig. 4.]

In some cases, the coagulum filling the cavity of the corpus luteum is absorbed, leaving a circular cup, whose margin is

fringed with small portions of the substance of the corpus luteum; this has been usually taken for a perfect corpus luteum, and preserved as such.

The cells met with in the ovaria before puberty are globular; but as the ovaria increase in size, the sides of these cells become squeezed, which gives them an oval form. [Plate VI. fig. 4. and Plate VII. fig. 2 and 3.]

EXPLANATION OF THE PLATES.

PLATE III.

In this Plate are represented six figures of the human ovary in a virgin state, and one of the human ovum before it is impregnated. The external appearances of the ovaria are of the natural size. The internal appearances are magnified two diameters, the ovum itself is magnified twenty-two diameters.

Fig. 1. the human ovary at twelve years of age, showing its external form.

Fig. 2. An internal view of the same: the blood vessels are injected, its structure is more compact than natural, the parts having been preserved in spirit. The cavities contained coagulated lymph and a serous fluid.

Fig. 3. The external view of the ovary at 20 years of age, after having been some days in spirit.

Fig. 4. An internal view of the same, showing that although the woman was a virgin, there was a corpus luteum arrived at its full growth containing an ovum, which has two membranous coverings, the amnion and chorion; the cavity surrounding the ovum was filled with blood; and the

ovum, by means of the chorion, has a slight attachment to the inner surface of the corpus luteum.

Fig. 5, represents the ovum in a detached state; it is smaller than that represented in my former paper, which had arrived at the cavity of the uterus, in the proportion of $\frac{1.5}{200}$ to $\frac{1.8}{200}$, and differs from it in being pellucid, the two opaque spots met with in the other not yet being formed.

Fig. 6. The external form of the ovarium at 47 years of age.

Fig 7. An internal view of the same, showing the remains of seven corpora lutea. The central cavities are distended with coagulated blood, as in all the corpora lutea met with after the ova that escaped from them had been impregnated. The corresponding ovarium contained five corpora lutea, similar in appearance to those here represented.

PLATE IV.

Contains five figures of the human ovarium after impregnation, the external appearances are of the natural size, the internal ones are magnified two diameters.

Fig. 1. The external appearance of the ovarium represented in a former paper on the human ovum, and shown again here to complete the series.

Fig. 2. An internal view of the same, to show the appearance of the corpus luteum five days after the impregnation of the ovum.

Fig. 3. The external appearance of the ovarium, six weeks after the impregnation of the ovum, which was arrested in the Fallopian tube.

Fig. 4. An internal view of the same, showing the corpus luteum partly absorbed, and the remaining parts unconnected with each other.

Fig. 5. A transverse section of the same ovarium, and its corpus luteum.

PLATE V.

Contains six figures of the human ovarium at different periods after impregnation, all of them of the natural size, except Fig. 6, which is magnified two diameters.

Fig. 1. An external view of the ovarium, that did not contain the ovum from which the child was produced, taken immediately after the child was born.

Fig. 2. An internal view of the same, in which there is a corpus luteum nearly arrived at its full size.

Fig. 3. The external view of the ovarium, in which the impregnated ovum had been formed.

Fig. 4. An internal view of the same, showing how much the corpus luteum had been broken down, and the want of distinctness in the remaining parts. There is also a new corpus luteum forming.

When Fig. 2 and Fig. 4 are compared, it will be seen, that all corpora lutea which have been preserved after the mother dies in child-birth, do not belong to the ovum of the child born, but to that which is to succeed it.

Fig. 5. The external appearance of the ovarium of a woman who had twelve children, and died at seventy years of age.

Fig. 6. Internal view of the same, to show how small the remains of the corpora lutea had become at that age. The large cyst contained coagulated lymph and serum.

PLATE VI.

Contains four figures of the ovaria of the cow. The external appearances of the natural size, the internal magnified two diameters.

Fig. 1. The ovarium of a calf, two months old.

Fig. 2. An internal view of the same.

Fig. 3. External view of the unimpregnated ovarium of a cow, 14 days after her being with calf.

Fig. 4. Internal view of the same, representing the commencement of a corpus luteum, and showing that the cells in the ovarium which before puberty are met with in a globular form, become gradually of an oval and irregular shape as the ovarium increases in size.

PLATE VII.

Contains three figures of the ovarium of a cow with calf. The external appearance is of the natural size ; and the internal is magnified two diameters.

Fig. 1. External view of the corresponding ovarium.

Fig. 2. Internal view of the same, showing the structure of the corpus luteum, which in this animal is very large, and is evidently made up of convolutions more nearly resembling those of the brain, than of any other organ.

Fig. 3. A transverse section of the corpus luteum.

PLATE VIII.

Contains six figures of the ovaria of the pig, in a virgin state. The external appearances of the natural size, the internal magnified two diameters.

Fig. 1. The external view of the ovarium at four months.

Fig. 2. Internal view of the same.

Fig. 3. External view of the ovarium at nearly six months.

Fig. 4. Internal view of the same, showing three incipient corpora lutea.

Fig. 5. External view at six months.

Fig. 6. Internal view, showing six corpora lutea, nearly if not quite at the full size: the cavities filled with blood, as in Fig. 1. 1. 1. 1. 1. 1. 1. but no ova were detected.

PLATE IX.

Contains six figures of ovaria of the pig; two in a virgin state, nearly six months old, in which the corpora lutea are in the process of degeneration, and four of the now after impregnation.

The first figure shows the ovarium, showing five incipient corpora lutea, spreading over the surface of the ovarium, and only beyond the orifices through which they pass, their inner surface is turned inside out, by which the outer surface has been completely discoloured.

Fig. 1. Internal view.

Fig. 1.

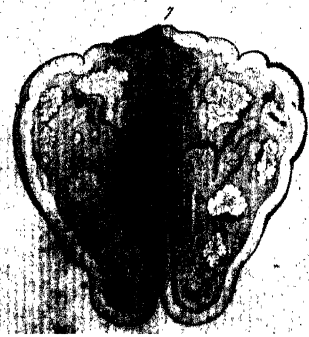
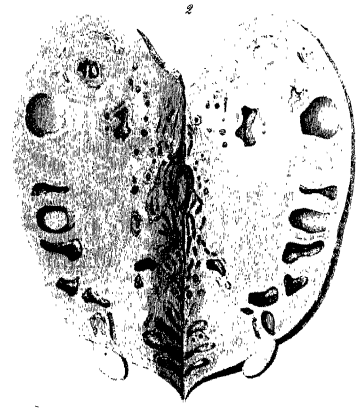
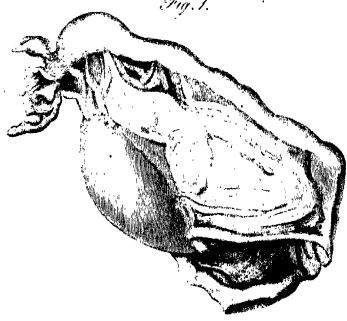
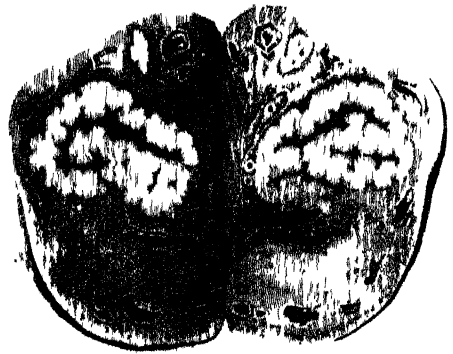
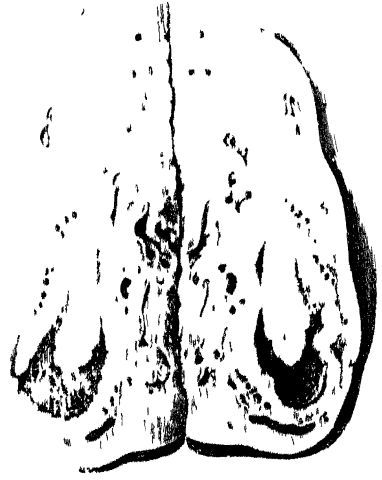


Fig. 1



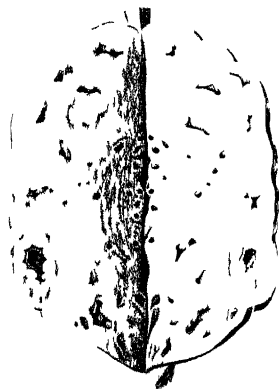
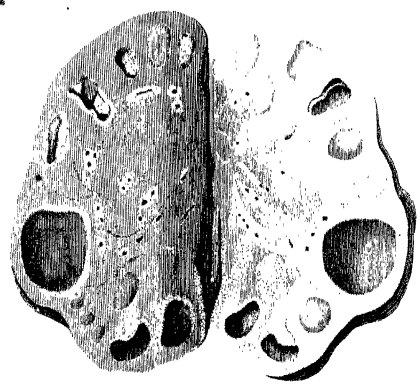
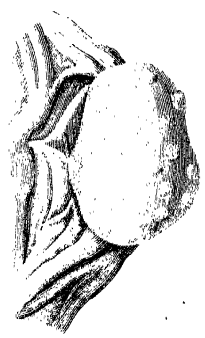
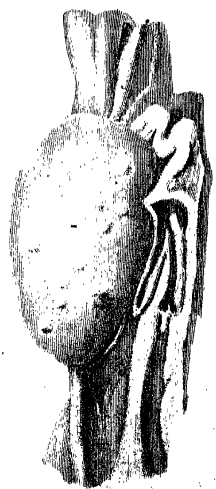


Fig. 1.



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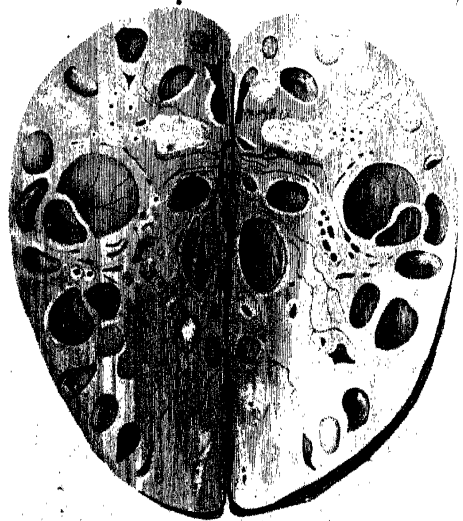
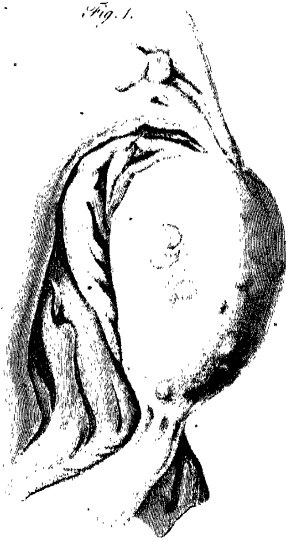
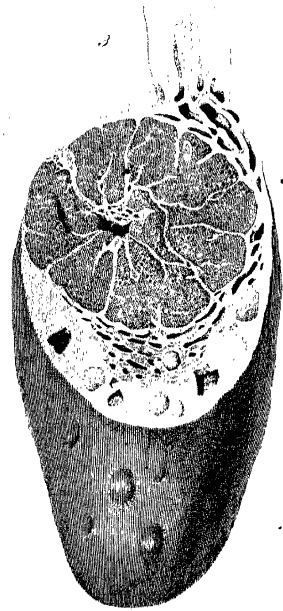


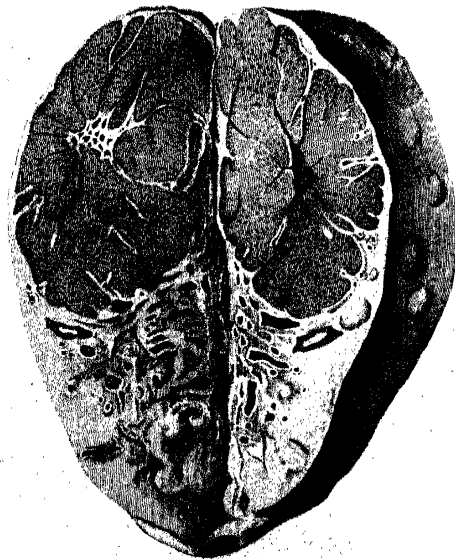
Fig. 1.

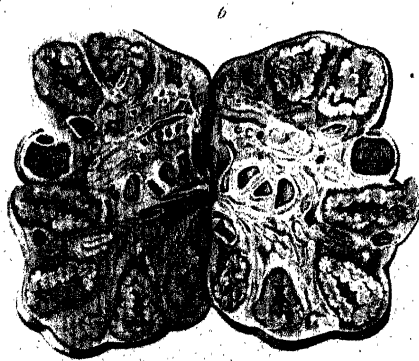
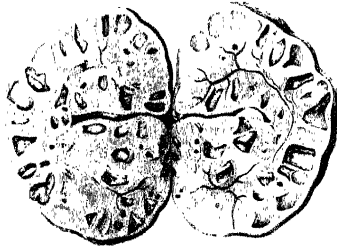


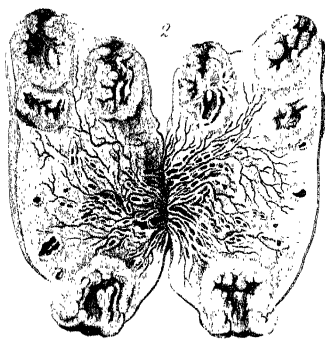
2



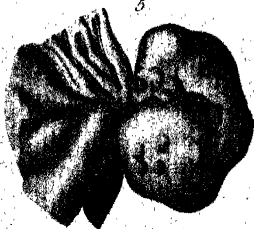
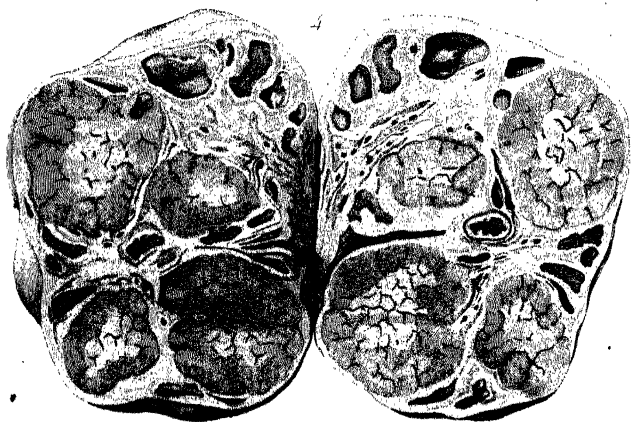
3







Natural Size.



Natural Size.

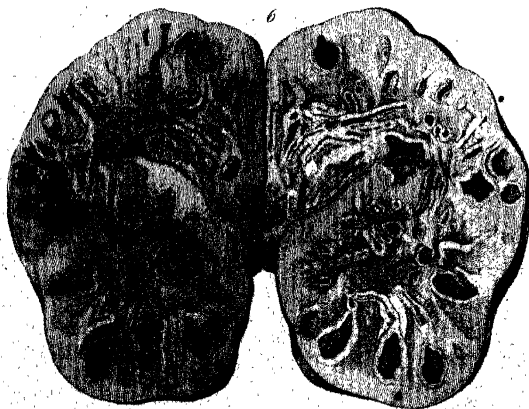


Fig. 3. An external view of the ovarium of a sow, 14 days after impregnation.

Fig. 4. The internal view, in which there are five corpora lutea, their appearance becoming very indistinct.

Fig. 5. The ovarium of a sow five years old, that had in all six litters of pigs, and had left off breeding for a year.

Fig. 6. An internal view of the same, to show that all vestiges of corpora lutea were nearly removed.

Since the first part of this Paper was printed, a foetus of the size usually met with at the end of the fourth month, has been found compleatly inclosed in the ovarium. It is evident that the ovum, after impregnation, was retained in the cavity of the corpus luteum in the manner explained above, since vestiges of the corpus luteum are still visible between the chorion and the substance of the ovarium. The mother died in consequence of hæmorrhage, produced by the bursting of a branch of the spermatic artery. An account of the case, with drawings of the parts made by Mr. BAUER, will be laid before the Society, after the long vacation, by Dr. GRANVILLE.

V. *Remarks on the probabilities of error in physical observations, and on the density of the earth, considered, especially with regard to the reduction of experiments on the pendulum. In a letter to Capt. HENRY KATER, F. R. S. By THOMAS YOUNG, M. D. For. Sec. R. S.*

Read January 21, 1819.

MY DEAR SIR,

THE results of some of your late experiments on the pendulum having led me to reflect on the possible inequalities in the arrangement of gravitating matter within the earth's substance, as well as on the methods of appreciating the accuracy of a long series of observations in general, I have thought that it might be agreeable to you, to receive the conclusions which I have obtained from my investigations, in such a form as might serve either to accompany the report of your operations, or to be laid before the Royal Society as a distinct communication.

1. *On the estimation of the advantage of multiplied observations.*

It has been a favourite object of research and speculation, among the authors of the most modern refinements of mathematical analysis, to determine the laws, by which the probability of occurrences, and the accuracy of experimental results, may be reduced to a numerical form. It is indeed true, that this calculation has sometimes vainly endeavoured to substitute arithmetic for common sense, and at other times has exhibited an inclination to employ the doctrine of chances as a sort of auxiliary in the pursuit of a political object, not

otherwise so easily attainable; but we must recollect, that at least as much good sense is required in applying our mathematics to objects of a moral nature, as would be sufficient to enable us to judge of all their relations without any mathematics at all: and that a wise government and a brave people may rely with much more confidence on the permanent sources of their prosperity, than the most expert calculators have any right to repose in the most ingenious combinations of accidental causes.

It is however an important, as well as an interesting study, to inquire in what manner the apparent constancy of many general results, which are obviously subject to great and numerous causes of diversity, may best be explained: and we shall soon discover that the combination of a multitude of independent sources of error, each liable to incessant fluctuation, has a natural tendency, derived from their multiplicity and independence, to diminish the aggregate variation of their joint effect; and that this consideration is sufficient to illustrate the occurrence, for example, of almost an equal number of dead letters every year in a general post office, and many other similar circumstances, which, to an unprepared mind, seem to wear the appearance of a kind of mysterious fatality, and which have sometimes been considered, even by those who have investigated the subject with more attention, as implying something approaching more nearly to constancy in the original causes of the events, than there is any just reason for inferring from them.

This statement may be rendered more intelligible by the simple case of supposing an equal large number of black and white balls to be thrown into a box, and 100 of them to be

drawn out either at once or in succession. It may then be demonstrated, as will appear hereafter, from the number of ways in which the respective numbers of each kind of balls may happen to be drawn, that there is 1 chance in $12\frac{1}{2}$ that exactly 50 of each kind may be drawn, and an even chance that there will not be more than 53 of either, though it still remains barely possible that even 100 black balls or 100 white may be drawn in succession.

From a similar consideration of the number of combinations affording a given error, it will be easy to obtain the probable error of the mean of a number of observations of any kind; beginning first with the simple supposition of the certainty of an error of constant magnitude, but equally likely to fall on either side of the truth, and then deducing from this supposition the result of the more ordinary case of the greater probability of small errors than of larger ones. This liability to a constant error may be represented, by supposing a counter to have two faces, marked 0 and 2; the mean value of an infinite number of trials will then obviously be 1, and the constant error of each trial will be 1, whether positive or negative.

Now in a combination of n trials with such a counter, if we divide the sum of the results by n , the greatest possible error of the mean thus found will be 1; and the probability of any other given error will be expressed by the number of combination of the faces of n counters affording that error, divided by the whole number of combinations; that is, by the corresponding coefficient of the binomial $(1 + 1)^n$, divided by 2^n , the sum of the coefficients. The calculation therefore will stand thus:

	$n = 2$			$n = 3$				$n = 4$					$n = 6$				$n = 8$				
Coefficients	1	2	1	1	3	3	1	1	4	6	4	1	1	6	15	20...	1	8	28	56	70...
Numbers thrown	0	2	4	0	2	4	6	0	2	4	6	8	0	2	4	6...	0	2	4	6	8...
Differences from n	2	0	2	3	1	1	3	4	2	0	2	4	6	4	2	0...	8	6	4	2	0...
Errors of the means	1	0	1	1	$\frac{1}{3}$	$\frac{1}{3}$	1	1	$\frac{1}{2}$	0	$\frac{1}{2}$	1	1	$\frac{2}{3}$	$\frac{1}{3}$	0...	1	$\frac{3}{4}$	$\frac{1}{2}$	$\frac{1}{4}$	0...
Sums of errors	1+0+1=2	1+1+1+1=4	1+2+0+2+1=6	1+4+5+0...	1+6+14+14+0...																
Mean errors	$\frac{2}{2} = \frac{1}{1}$	$\frac{4}{2} = \frac{1}{2}$	$\frac{6}{2} = \frac{1}{3}$	$\frac{20}{6} = \frac{5}{3}$	$\frac{70}{28} = \frac{5}{4}$																

It is easy to perceive that these coefficients must express the true numbers of the combinations, since they are formed by adding together the two adjacent members of the preceding series; thus when n is 3, 1 combination giving the number 0 and 3 the number 2, these two combinations, being again respectively combined with 2 and 0 of a fourth counter, give $1 + 3 = 4$, for the combinations affording the number 2 in the next series; while each succeeding series must continue to begin and end with unity, since there is only one combination that can afford either of the extremes.

In order to continue the calculation with greater convenience, we must find a general expression for the middle terms, 2, 6, 20, 70..., neglecting the odd values of n . The first, 2, is made up of $(1 + 1)$, the second, 6, is $2(2 + 1)$; 20 is $2(6 + 4)$ and $70 = 2(20 + 15)$: or $6 = 2(2 \cdot \frac{3}{2})$, $20 = 2(6 \cdot \frac{5}{3})$, $70 = 2(20 \cdot \frac{7}{4})$, whence the series may easily be continued at pleasure, multiplying always the preceding term by $\frac{6}{2}$, $\frac{10}{3}$, $\frac{14}{4}$, $\frac{18}{5}$,... We have also $6 = 16 \cdot \frac{3}{8} = 2^4 \cdot \frac{3}{8}$, $20 = 2^6 \cdot \frac{5}{16}$, and $70 = 2^8 \cdot \frac{35}{128}$: consequently the terms of this series, divided by 2^n , will always express the mean errors already calculated. From this value of the middle term we may easily deduce that of the neighbouring terms by means of the original formula $n \cdot \frac{n-1}{2} \dots \frac{\frac{1}{2}n+1}{\frac{1}{2}n} \cdot \frac{\frac{1}{2}n}{\frac{1}{2}n+1} \cdot \frac{\frac{1}{2}n-1}{\frac{1}{2}n+2} \dots$; the

first factor less than unity being always $\frac{\frac{1}{2}n}{\frac{1}{2}n+1} = \frac{1}{1+\frac{2}{n}}$. The magnitude of the mean error is exhibited in the annexed table.

n Mean error	
2 .500000	The general expression for this series being $\frac{1}{2} \cdot \frac{3}{4} \cdot \frac{5}{6} \dots \frac{n-1}{n}$, it is obvious that if we
4 .375000	multiply it by $\frac{2}{3} \cdot \frac{4}{5} \cdot \frac{n-2}{n-1}$, the product will be
6 .312500	$\frac{1}{n}$, whatever the value of n may be: and
8 .273437	when that value is large, the factors of these
10 .246094	two expressions will approach so near to
12 .225586	each other that they may be considered as
14 .209473	equal; consequently the corresponding terms
16 .196381	of either, taken between any two large va-
18 .185471	lues of n , will vary in the subduplicate ratio
20 .176196	of n , since their product, which may be con-
30 .144466	sidered as the square of either, varies in the
40 .125363	simple ratio of n , so that the mean error may
50 .112271	ultimately be expressed by $\sqrt{\frac{1}{pn}}$. The va-
60 .102574	lue of p evidently approximates to that of
70 .095022	the quadrant of a circle, of which the radius
80 .088924	is unity: thus for $n=10$ it is 1.6512, and for
90 .083868	$n=100$, 1.5788, instead of 1.5708; and the
100 .079586	ultimate identity of these magnitudes has
	been demonstrated by EULER and others. (See Mr. HER-
	SCHEL's Treatise on Series, in LACROIX, Engl. Ed. n. 410.)

The fraction thus found, multiplied by 2^n , gives the number of combinations expressed by the middle term, in which the error vanishes, when n is even: and the whole number of

combinations being also 2^n , it is obvious that the fraction alone must express the probability of a result totally free from error. The neighbouring terms on each side, for $n = 100$, are .078025, .073524, and .066588, the sum of the 7 being .515860; and since this sum exceeds $\frac{1}{2}$, it is obviously more probable that the result of 100 trials will be found in some of these seven terms, than in any of the remaining 94, and that the mean error will not exceed $\frac{3}{50}$. When n is so large, that the terms concerned may be considered as nearly equal, the factors $\frac{\frac{1}{2}n}{\frac{1}{2}n+1}$, $\frac{\frac{1}{2}n-1}{\frac{1}{2}n+2}$..., may be expressed by $1 - \frac{2}{n}$, $1 - \frac{6}{n}$, $1 - \frac{10}{n}$..., and the terms themselves by 1 , $1 - \frac{2}{n}$, $1 - \frac{8}{n}$, $1 - \frac{18}{n}$... the negative parts forming the series $\frac{2}{n}(1, 4, 9 \dots)$ of which the sum, for q terms, is $\frac{2}{n}(\frac{1}{3}q^3 + \frac{1}{2}q^2 + \frac{1}{6}q)$ or ultimately $\frac{2}{3n}q^3$; consequently if we call the middle term e , we must determine q in such a manner as to have $e(2q - \frac{4}{3n}q^3) = \frac{1}{2} - e$, and $q(1 - \frac{2}{3n}q^2) = \frac{1}{4e} - \frac{1}{2}$; but e has been already found, in this case, $= \sqrt{\frac{1}{pn}}$, and neglecting at first the square of q , we have $q = \frac{1}{4} \sqrt{(pn)} - \frac{1}{2}$, and $q^2 = \frac{1}{16}pn$, whence $\frac{2}{3n}q^3 = \frac{1}{24}p$, and $1 - \frac{2}{3n}q^2 = .93455$; hence, for a second approximation, $.93455 q = \frac{1}{4e} - \frac{1}{2}$, and $q = .2674 \sqrt{(pn)} - .53$; and by continuing the operation we obtain $.9235 q = \frac{1}{4e} - \frac{1}{2}$, and $q = .271 \sqrt{(pn)} - .54$; consequently the probable error, being expressed by $\frac{2q}{n}$, will be $.542 \sqrt{\frac{p}{n}} - \frac{1.08}{n} = \frac{.679}{\sqrt{n}} - \frac{1.08}{n}$. This formula, for $n = 100$, becomes .0571, and for $n = 10000$, .00679 — .00011 = .00668.

We must not, however, lose sight, in this calculation, of the original condition of liability to a certain constant error in each trial. For example, we may infer from it, that if we made 100 observations of the place of a luminary, each differing $1'$ from the truth, but indifferently on either side of it, the error of the mean result would probably not exceed $\frac{3}{50} \cdot 1' = 3.6''$; and that in 1000 observations it would probably be reduced to about a second. Now although, in the methods of observing which we employ, the error is liable to considerable variations, yet it may be represented with sufficient accuracy, by the combination of two or more experiments in which the simpler law prevails. For example, the combination of two counters, such as have been considered, is equivalent to the effect of a die with four faces, or a tetraedron, marked 0, 2, 2, and 4, or with errors expressed by 1, 0, 0, and -1 ; the combination of three counters is represented by a die having eight faces, or an octaedron, with the errors 1, $\frac{1}{3}$, $\frac{1}{3}$, $\frac{1}{3}$, $-\frac{1}{3}$, $-\frac{1}{3}$, $-\frac{1}{3}$, -1 ; and the combination of four, by a solid of 16 sides, with the errors 1, $4 \times \frac{1}{2}$, 6×0 , $4 \times -\frac{1}{2}$, -1 . These distributions evidently resemble those which are generally found to take place in the results of our experiments; and it is of the less consequence to represent them with greater accuracy, since the minute steps, by which the scale of error varies, have no sensible effect on the result, especially when the number of observations is considerable. If, for example, instead of two trials with the tetraedron, having the errors 1, 0, 0, -1 , we made two trials with a solid of 21 faces, having the errors distributed equally from 1, .9, .8 .. to -1 , the mean error of all the possible combinations would only

vary from .375 to .349; and in a greater number of trials the errors would approach still nearer to equality.

Now in order to employ any of these suppositions for the purpose of calculation, it is only necessary to compute the corresponding mean error, and to make it equal to the actual mean error of a great number of observations. Thus, if we consider each observation as representing a binary combination of counters or constant errors, in which the mean error is $\frac{1}{2}$, and adding together the differences of the several results from the mean, and dividing by their numbers, we find the mean error of 100 observations $1'$, we must consider the original constant error as equal to $2'$, which is to be made the unit for 200 primitive combinations; and $\frac{.679}{\sqrt{200}} - \frac{1.08}{200} = .0426$; and the probable error of the mean will be $.0426 \times 120 = 5.1''$. For a quaternary combination, if the error, which amounts to $\frac{3}{8}$, be found $1'$, the unit will be $\frac{8}{3}'$, and for $n = 400$, we have $.03125 \times \frac{8}{3}' = 5.0''$. And if we set out with a large number m of combinations, the mean error being $\sqrt{\frac{1}{pm}} = e$, the unit will be $e \sqrt{(pm)} = 1$, and the probable error of nm trials being equal to this unit multiplied by $.542 \sqrt{\frac{p}{nm}}$, neglecting the very small fraction $\frac{1.08}{nm}$, we have $.542 \sqrt{\frac{p}{nm}} e \sqrt{(pm)} = .542 p \sqrt{\frac{1}{n}} e = .8514 \sqrt{\frac{1}{n}} e$: which, if e be $1'$, and $n = 100$, gives again $5.1''$. It appears therefore that the supposition, respecting the number of combinations representing the scale of error, scarcely makes a perceptible difference in the result, after the exclusion of the constant error: and that we may safely represent the probable error of the mean result of n observations, by the expression $.85 \frac{e}{\sqrt{n}}$, e being the mean of all the actual errors.

We might obtain a conclusion nearly similar by considering the sum of the squares of the errors, amounting always to $n \cdot 2''$: but besides the greater labour of computing the sum of the squares of the errors of any series of observations, the method, strictly speaking, is somewhat less accurate, since the amount of this sum is affected in a slight degree by any error which may remain in the mean, while the simple sum of the errors is wholly exempted from this uncertainty. In other respects the results here obtained do not materially differ from those of LEGENDRE, BESSEL, GAUSS, and LAPLACE: but the mode of investigation appears to be more simple and intelligible.

It may therefore be inferred from these calculations, first, that the original conditions of the probability of different errors, though they materially affect the observations themselves, do not very greatly modify the nature of the conclusions respecting the accuracy of the mean result, because their effect is comprehended in the magnitude of the mean error from which those conclusions are deduced: and secondly, that the error of the mean, on account of this limitation, is never likely to be greater than six sevenths of the mean of all the errors, divided by the square root of the number of observations. But though it is perfectly true, that the probable error of the mean is always somewhat less than the mean error divided by the square root of the number of observations, provided that no constant causes of error have existed; it is still very seldom safe to rely on the total absence of such causes; especially as our means of detecting them must be limited by the accuracy of our observations, not assisted, in all instances, by the tendency to equal errors on either side of the truth: and when we are comparing a series

of observations made with any one instrument, or even by any one observer, we can place so little reliance on the absence of some constant cause of error, much greater than the probable result of the accidental causes, that it would in general be deceiving ourselves even to enter into the calculation upon the principles here explained: and it is much to be apprehended, that for want of considering this necessary condition, the results of many elegant and refined investigations, relating to the probabilities of error, may in the end be found perfectly nugatory.

These are cases in which some little assistance may be derived from the doctrine of chances with respect to matters of literature and history: but even here it would be extremely easy to pervert this application in such a manner, as to make it subservient to the purpose of clothing fallacious reasoning in the garb of demonstrative evidence. Thus if we were investigating the relations of two languages to each other, with a view of determining how far they indicated a common origin from an older language, or an occasional intercourse between the two nations speaking them, it would be important to inquire, upon the supposition that the possible varieties of monosyllabic or very simple words must be limited by the extent of the alphabet to a certain number; and that these names were to be given promiscuously to the same number of things, what would be the chance that 1, 2, 3 or more of the names would be applied to the same things in two independent instances.

Now we shall find, upon consideration, that for n names and n things, the whole number of combinations, or rather permutations of the whole nomenclature would be $m = 1.2.$

$3 \dots n$; and that of these the number in which no one name agreed would be $a_n = m - a_1 - n \cdot \frac{n-1}{2} \cdot a_2 - n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot a_3 \dots - n \cdot a_{n-1}$; each term expressing the number of agreements in $n, n-1, n-2 \dots$ instances only, and being made up of all the combinations of so many out of n things, each occurring as many times as all the remaining ones can disagree. Hence we may easily obtain the successive values of a from each other, the first being obviously 1, as a single name can only be given in one way to a single thing, therefore,

$$a_1 = 1$$

$$a_2 = 2 - 1 = 1$$

$$a_3 = 6 - 1 - 3 = 2$$

$$a_4 = 24 - 1 - 6 - 8 = 9$$

$$a_5 = 120 - 1 - 10 - 20 - 45 = 44$$

$$a_6 = 720 - 1 - 15 - 40 - 135 - 264 = 265$$

$$a_7 = 5040 - 1 - 21 - 70 - 315 - 924 - 1855 = 1854$$

$$a_8 = 40320 - 1 - 28 - 112 - 630 - 2464 - 7420 - 14832 = 14833$$

$$a_9 = 362880 - 1 - 36 - 168 - 1134 - 5544 - 22260 - 66744 - 133497 = 133496$$

$$a_{10} = 3628800 - 1 - 45 - 240 - 1890 - 11088 - 55650 - 222480 - 667485 - 1334961 = 1334961$$

From this computation it may be inferred, that, for 10 names, the probabilities will stand thus:

No coincidence	.367880	One or more	.632120
One only	.367880	Two or more	.264240
Two only	.183941	Three or more	.080300
Three only	.061309	Four or more	.018991
Four only	.015336	Five or more	.003655
Five only	.003056	Six or more	.000599
Six only	.000521	Seven or more	.000078

Seven only	.000066	Eight or more	.000012
Eight only	.000012	Nine or Ten	.0000003

The same results may be still more readily obtained from the supposition that n is a very large number; for then, the probability of a want of coincidence for a single case being $\frac{n-1}{n}$, the probability for two trials will be $\left(\frac{n-1}{n}\right)^2$, and for the whole n , $\left(\frac{n-1}{n}\right)^n = \left(1 - \frac{1}{n}\right)^n$: but the hyperbolical logarithm of $1 - \frac{1}{n}$ being ultimately $-\frac{1}{n}$, that of $\left(1 - \frac{1}{n}\right)^n$ will be -1 , consequently the probability of no coincidence will be $\frac{1}{2.718282} = .3678794$: and if n is increased by 1, each of these cases of no coincidence will afford 1 of a single coincidence: if by two, each will afford one of a double coincidence, but half of them will be duplicates; and if by three, the same number must be divided by 6, since all the combinations of three would be found six times repeated. We have therefore for

No coincidence	.3678794	One or more	.6321206 = $\frac{2}{3} -$
One only	.3678794	Two or more	.2642412 = $\frac{1}{4} +$
Two only	.1839397	Three or more	.0803015 = $\frac{1}{12} -$
Three only	.0613132	Four or more	.0189883 = $\frac{1}{53}$
Four only	.0153283	Five or more	.0036600 = $\frac{1}{273}$
Five only	.0030657	Six or more	.0005943 = $\frac{1}{1683}$
Six only	.0005109	Seven or more	.0000834 = $\frac{1}{12000}$
Seven only	.0000730	Eight or more	.0000105 = $\frac{1}{95200}$

It appears therefore that nothing whatever could be inferred with respect to the relation of two languages from the coincidence of the sense of any single word in both of them; and that the odds would only be 3 to 1 against the agreement of two words: but if three words appeared to be identical, it

would be more than 10 to 1 that they must be derived in both cases from some parent language, or introduced in some other manner; six words would give near 1700 chances to 1, and 8 near 100,000: so that in these last cases the evidence would be little short of absolute certainty.

In the Biscayan, for example, or the ancient language of Spain, we find in the vocabulary accompanying the elegant essay of Baron W. VON HUMBOLDT, the words *berria*, new; *ora*, a dog; *guchi*, little; *oguia*, bread; *otsoa*, a wolf, whence the Spanish *onza*; and *zazpi*, or, as LACROZE writes it, *shashpi*, seven. Now in the ancient Egyptian, new is *BERI*; a dog, *UHOR*; little, *KUDCHI*; bread, *OIK*; a wolf, *UONSH*; and seven, *SHASHF*; and if we consider these words as sufficiently identical to admit of our calculating upon them, the chances will be more than a thousand to one, that, at some very remote period, an Egyptian colony established itself in Spain: for none of the languages of the neighbouring nations retain any traces of having been the medium through which these words have been conveyed.

On the other hand, if we adopted the opinions of a late learned antiquary, the probability would be still incomparably greater that Ireland was originally peopled from the same mother country: since he has collected more than 100 words which are certainly Egyptian, and which he considers as bearing the same sense in Irish; but the relation, which he has magnified into identity, appears in general to be that of a very faint resemblance: and this is precisely an instance of a case, in which it would be deceiving ourselves to attempt to reduce the matter to a calculation.

The mention of a single number, which is found to be in-

disputably correct, may sometimes afford a very strong evidence of the accuracy and veracity of a historian. If the number were indefinitely large, the probability that it could not have been suggested by accident would amount to an absolute certainty: but where it must naturally have been confined within certain moderate limits, the confirmation, though somewhat less absolute, may still be very strong. For example, if the subject were the number of persons collected together for transacting business, it would be a fair presumption that it must be between 2 or 3 and 100, and the chances must be about 100 to 1 that a person reporting it truly must have some good information; especially if it were not an integral number of tens or dozens, which may be considered as a species of units. Now it happens that there is a manuscript of DIODORUS SICULUS, which, in describing the funerals of the Egyptians, gives 42 for the number of persons who had to sit in judgment on the merits of the deceased: and in a multitude of ancient rolls of papyrus, lately found in Egypt, it may be observed, that 42 personages are delineated, and enumerated, as the judges assisting Osiris in a similar ceremony. It is therefore perfectly fair to conclude from this undeniable coincidence, that we might venture to bet 100 to 1, that the manuscript in question is in general more accurate than the others which have been collated; that DIODORUS SICULUS was a well informed and faithful historian; that the graphical representations and inscriptions in question do relate to some kind of judgment; and lastly, that the hieroglyphical numbers, found in the rolls of papyrus, have been truly interpreted.

2. On the mean density of the earth.

It has been observed by some philosophers, that the excess of the density of the central parts of the earth, above that of the superficial parts, is so great as to render it probable that the whole was once in a state of fluidity, since this is the only condition that would enable the heaviest substances to sink towards the centre. But before we admit this inference, we ought to inquire, how great would be the effect of pressure only in augmenting the mean density, as far as we can judge of the compressibility of the substances, which are the most likely to be abundant, throughout the internal parts of the structure.

Supposing the density at the distance x from the centre to be expressed by y , the fluxion dy will be jointly proportional to the thickness of the elementary stratum, or to its fluxion $-dx$, to the actual density y , and to the attraction of the interior parts of the sphere, which varies as $\frac{\int yxxdx}{xx}$; since the increment of pressure, and consequently that of density, depends on the combination of these three magnitudes: we have therefore $-ndy = ydx \frac{\int ydx}{xx}$; an equation which will readily afford us the value of y in a series of the form $1 + ax^2 + bx^4 + \dots$.

In order to determine the coefficients, we must first find $\frac{xxdx}{xx} = \frac{1}{3}x + \frac{1}{5}ax^3 + \frac{1}{7}bx^5 + \dots$, and multiplying this by $(1 + ax^2 + bx^4 + \dots) dx$, we obtain

$$\begin{aligned} -ny &= -n - nax^2 - nbx^4 - ncx^6 \\ &= C + \frac{1}{2.3}x^2 + \frac{1}{4.5}a \left. \begin{array}{l} x^4 \\ + \frac{1}{3.4}a \end{array} \right\} + \frac{1}{6.7}b \left. \begin{array}{l} x^6 \\ + \frac{1}{5.6}a^2 \\ + \frac{1}{3.6}b \end{array} \right\} x^6 + \dots \end{aligned}$$

Hence, by comparing the corresponding terms, we obtain

$$\begin{aligned}
 C &= -n; \\
 a &= -.1666667n^{-1} \text{ Logarithm, } 9.2218487 \\
 b &= .2222222n^{-2} \quad 8.3467875 \\
 c &= -.00268960n^{-3} \quad 7.4296867 \\
 d &= .000308154n^{-4} \quad 6.4887650 \\
 e &= -.0000340743n^{-5} \quad 5.5324269 \\
 f &= .00000367495n^{-6} \quad 4.5652514 \\
 g &= -.000000389086n^{-7} \quad 3.5911459 \\
 [h &= .00000004062n^{-8} \quad 2.6087] \\
 [i &= -.00000000420n^{-9} \quad 1.6232] \\
 [k &= -.00000000043n^{-10} \quad 0.6335]
 \end{aligned}$$

After the exact determination of the first seven coefficients, the next three are obtained with sufficient accuracy by means of the successive differences of the logarithms, compared with those of the natural numbers.

It happens very conveniently, that the conditions of the problem are such, as to afford a remarkable facility in deriving from this series another, which is much more convergent, and which gives us the hyperbolic logarithm of y ; for since $-n \frac{dy}{y} = dx \frac{f y x dx}{x x}$, and $\frac{f y x dx}{x x} = \frac{1}{3} x + \frac{1}{5} a x^3 + \frac{1}{7} b x^5 + \dots$, if we multiply this by dx , and take the fluent, we shall have $HLy = -\frac{1}{n} \left(\frac{1}{2.3} x^2 + \frac{1}{4.5} a x^4 + \frac{1}{6.7} b x^6 + \dots \right)$.

We may determine the degree of compressibility corresponding to a given value of n , by comparing the equation $-n \frac{dy}{y} = dx \frac{f y x dx}{x x}$, or $= dx p$, with the properties of the modulus of elasticity M , which is the height of such a column of the given substance, that the increment of density y' , occasioned by the additional weight of the increment x' , is always

to y , as x' to M , or $\frac{y'}{y} = \frac{x'}{M}$, whence $-\frac{dy}{y} = \frac{dx}{M}$; consequently in the present case we have $\frac{dxp}{n} = \frac{dx}{M}$; and $M = \frac{n}{p}$: and if we make $x = 1$ in the value of p , we shall obtain M in terms of the radius of the earth, considered as unity. When y is invariable, and n infinite, the density being uniform, p becomes $\frac{1}{3}$, and the mean density will always be expressed by $3p$, since the attractive force is simply as the mean density: and if we divide $3p$ by y , we shall have the relation of the mean density to the superficial density. The results of this calculation, for different values of n , are arranged in the table, which will be found sufficiently accurate for the purposes of the investigation, though not always correct to the last place of figures.

n	p	$M = \frac{n}{p}$	$3p$, mean density		$y \cdot \frac{3p}{y}$, comp. den.	
∞	.33333	∞	1.0000 = 1:1.0000		1.000	1.000
1	.30290	3.301	.9087	1.1005	.855	1.065
$\frac{1}{2}$.27735	1.803	.8320	1.2019	.738	1.127
$\frac{1}{3}$.25535	1.305	.7660	1.3054	.646	1.185
$\frac{1}{4}$.23688	1.055	.7106	1.4071	.575	1.24
$\frac{1}{5}$.22058	.907	.6617	1.5111	.510	1.30
$\frac{1}{6}$.20616	.808	.6185	1.6168	.458	1.35
$\frac{1}{7}$.194	.736	.582	1.72	.419	1.40
$\frac{1}{8}$.183	.681	.549	1.82	.377	1.45
$\frac{1}{9}$.172	.646	.516	1.94	.346	1.49
$[\frac{1}{10}]$.162	.617	.486	2.05	.320	1.52]
$[\frac{1}{11}]$.153	.594	.459	2.16	.298	1.55]
$[\frac{1}{12}]$.145	.575	.435	2.28	.28	1.57]
$[\frac{1}{20}]$.1	.5	.3	3.3	.17	1.8]

The reciprocals of the mean density are inserted, on account

of the simplicity of the progression which they exhibit, being in the first instance precisely equal to $1 + \frac{1}{10^n}$, and varying but slowly from this value.

Now if we suppose, with Mr. LAPLACE, the mean density of the earth to be to that of the superficial parts as 1.55 to 1, it appears from this table, that the height of the modulus of elasticity must be about .594; that is, more than 12 million feet, while the modulus of the hardest and most elastic substances, that have been examined, amounts only to about 10 million. It follows therefore, that the general law, of a compression proportionate to the pressure, is amply sufficient to explain the greater density of the internal parts of the earth; and the fact demonstrates, that this law, which is true for small pressures in all substances, and with regard to elastic fluids, in all circumstances, requires some little modification for solids and liquids, the resistance increasing somewhat faster than the density: for no mineral substance is sufficiently light and incompressible to afford a sphere of the magnitude of the earth, and of so small a specific gravity, without some such deviation from the general law. A sphere of water would be incomparably more dense, and one of air would exceed this in a still greater proportion: indeed, even the moon, if she is really perforated, as has sometimes been believed, and contains cavities of any considerable depth, would soon have absorbed into her substance the whole of her atmosphere, supposing that she ever had one. It may be objected, that the resistance of solids to actual compression may possibly be considerably greater than appears in our experiments, since we are not absolutely certain that they

do not extend in a transverse direction, when we compress them in a longitudinal one, as is obviously the case with some soft elastic substances: but this objection is removed by the experiment on the sound of ice, which affords, either accurately or very nearly, the same resistance to compression as a portion of water confined in a strong vessel; and this it could not do, if the particles of ice were allowed to expand laterally under the operation of a compressing force.

Mr. LAPLACE's conclusion, respecting the precise proportion of the densities, is indeed derived from another supposition respecting their variation, and would be somewhat modified by the adoption of this theory; it would not, however, be so materially altered, as by any means to invalidate the general inference. It would therefore be proper to revise the calculations derived from the lunar motions and the ellipticity of the earth, and to employ in them a variation of density somewhat resembling that which is here investigated. Indeed without reference to the effects of compressibility, it is obviously probable that the density of the earth should vary more considerably in a given depth towards the surface than near the centre, although the calculation, upon Mr. LAPLACE's more simple hypothesis, of a uniform variation, is much less intricate. It would however be justifiable, as a first approximation, to reject those terms of the series which would vanish if n and x were very small, and to make $y = 1 + ax^2$; and indeed this formula has in one respect an advantage over the series, as it seems to approach more nearly to the law of nature, in expressing a resistance somewhat greater towards the centre, where the density is most augmented: we have then, if the superficial density be to the

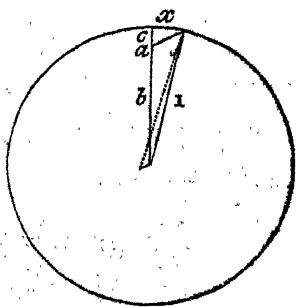
mean as 1 to q , $q = \frac{1+\frac{1}{2}a}{1+a}$, whence $a = -\frac{q-1}{q-.6}$; and if $q = 1.55$, $a = -.58$, affording an expression which is, in all probability, accurate enough for every astronomical purpose.

If the variation of density were supposed to proceed equably with the variation of quantity, it would obviously be as the square of the distance from the centre, and the density would be as $1 - ax^3$, the mean density being found at the surface of a sphere containing half as much as the whole earth; and this might be considered as the most natural hypothesis, if we disregarded the effects of compression: but the arithmetical progression of densities, from the centre to the surface, seems in every way improbable.

3. On the irregularities of the earth's surface.

A. If we suppose the plumb line to deviate from its general direction on account of the attraction of a circumscribed mass, situated at a moderate depth below the earth's surface, the distance of the two points of greatest deviation from each other will be to the depth of the attracting point as 2 to $\sqrt{2}$.

Let the magnitude of the additional mass be to that of the earth as a to 1, and let its distance from the centre be b ; then supposing the earth a sphere, and its radius unity, and calling the angular distance of any point from the semidiameter passing through the mass x , the linear distance from the mass will be $\sqrt{(f^2x + (\zeta x - b)^2)} = \sqrt{(f^2x + \zeta^2x - 2b\zeta x + b^2)} = \sqrt{(1 + b^2 - 2b\zeta x)}$; consequently the disturbing attraction will be $\frac{a}{1 + bb - 2b\zeta x}$: but the sine of the angle subtended by the two



centres of attraction will be to their distance b as fx to the oblique distance $\sqrt{(1 + b^2 - 2b\zeta x)}$; it will therefore be expressed by $\frac{bf x}{\sqrt{(1 + bb - 2b\zeta x)}}$; and the sine of the very small angular deviation of the joint force from the radius will be to the line measuring the disturbing force as this last sine to the radius, the difference of the third side of the triangle from the radius being inconsiderable; consequently the deviation will be every where expressed by $\frac{abfx}{(1 + bb - 2b\zeta x)^{\frac{3}{2}}} = d$. Now in order to find where this is greatest, we must make its fluxion vanish, and $0 = \frac{\zeta x dx}{(1 + bb - 2b\zeta x)^{\frac{3}{2}}} - \frac{3}{2} \cdot \frac{fx 2b\zeta x dx}{(1 + bb - 2b\zeta x)^{\frac{5}{2}}}$, $\zeta x (1 + b^2 - 2b\zeta x) = 3bf^2 x$, $3b\zeta^2 x - 2b\zeta^3 x + (1 + b^2) \zeta x = 3b$, $\zeta^2 x + \frac{1+bb}{b} \zeta x = 3$, and $\zeta x = \sqrt{3 + \left[\frac{1+bb}{2b}\right]^2} - \frac{1+bb}{2b}$; but, making $b = 1 - c$, $\frac{1+bb}{2b}$ becomes $\frac{1+1-2c+cc}{2b} = 1 + \frac{cc}{2b}$; and c being very small, ζx will be $\sqrt{4 + \frac{cc}{b}} - 1 - \frac{cc}{2b} = 2 + \frac{cc}{4b} - 1 - \frac{cc}{2b} = 1 - \frac{cc}{4b}$; whence $fx = \sqrt{(1 - [1 - \frac{cc}{4b}]^2)} = \sqrt{1 - 1 + \frac{cc}{2b}} = \sqrt{\frac{c}{2b}}$. or simply $\sqrt{\frac{1}{2}c}$, and $c = \sqrt{2fx}$.

B. The sine of the greatest deviation of the plumb line will amount to $d = .385 \frac{a}{cc}$, a being the disturbing mass, and c its depth.

Since $\zeta x = 1 - \frac{cc}{4b}$, $2b\zeta x = 2b - \frac{cc}{2}$, and $1 + bb - 2b\zeta x + \frac{cc}{2} = (1 - b)^2 + \frac{cc}{2} = c^2 + \frac{cc}{2} = \frac{3}{2}c^2$; and $abfx$ becomes $\frac{abc}{\sqrt{(2b)}}$, whence $d = \frac{abc}{\sqrt{(2b \cdot 27)^{\frac{3}{2}}}} = \frac{2a\sqrt{b}}{\sqrt{27}cc} = .385 \frac{a\sqrt{b}}{cc}$, or simply $.385 \frac{a}{cc}$; also $a = 2.618c^2d$, and $c = \sqrt{.385 \frac{a}{d}}$. If the density were doubled throughout the extent of a sphere touching the surface internally, the radius being c , we should

have $a = c^3$ and $d = .385c$, and $c = 2.6d$: but this is a much greater increase of density than is likely to exist on a large scale: so that c must probably in all cases be considerably greater than this.

C. The greatest elevation of the general surface above the sphere will be $\frac{a}{c}$, on the supposition that the mutual attraction of the elevated parts may safely be neglected.

The fluxion of the elevation is as the fluxion of the arc and as the deviation d conjointly; it will therefore be expressed by $\frac{abf\dot{x}dx}{(1+bb-zb\zeta x)^{\frac{3}{2}}}$. Now the fluxion of $\frac{1}{\sqrt{1+bb-zb\zeta x}}$ is $-\frac{1}{2} \frac{zb\zeta \dot{x}dx}{(1+bb-zb\zeta x)^{\frac{3}{2}}}$, consequently the fluent of the elevation will be $\frac{-a}{\sqrt{1+bb-zb\zeta x}}$: and while ζx varies from 1 to -1 , this fluent will vary from $\frac{-a}{1-b}$ to $\frac{-a}{1+b}$, the difference being $a \left(\frac{1}{1-b} - \frac{1}{1+b} \right) = a \left(\frac{1}{c} - \frac{1}{2-c} \right) = a \left(\frac{2-c-c}{2c-cc} \right)$, or simply $\frac{a}{c}$, since c is an extremely small fraction. This quantity comprehends indeed the depression on the remoter side of the sphere, which would be required to supply matter for the elevation; but it is obvious that such a depression must be wholly inconsiderable.

D. The diminution of gravity to the centre at the highest point is $\frac{2a}{c}$, while the increase from the attraction of the disturbing mass is nearly $\frac{a}{cc}$, which is greater in the proportion that half the radius bears to c .

E. The increase of gravity, at the point of greatest deviation, is to the deviation itself, or its sine d , as $\sqrt{2}$ to 1.

For the deviation is the measure of the horizontal attraction of the disturbing mass, which is to its vertical attraction

as fx to c , or as $\sqrt{\frac{1}{2}}$ to 1. Thus if d were $5''$, or $\frac{5}{206265}$, the horizontal force would be $\frac{7.071}{206265} = \frac{1}{29170}$, and the acceleration of a pendulum $\frac{1}{58340}$ or $1.5''$ of time in a day. It is true that a part of the deviation might depend on a defect of density as well as on an excess; but this defect could not amount to any great proportion of the whole, while the excess above the general density might easily be much more considerable, so that the acceleration of the pendulum could scarcely be *less* than a second in a day, if the greatest deviation of the plumb line were $5''$; and if the deviation were $5''$ at any other place, there would be a *greater* acceleration than a second at a point more or less remote from it.

F. If there were an excess of density on one side, and a deficiency on the other, so as to constitute virtually two centres of attraction and repulsion, and supposing their distances to be equal, and such as to produce the greatest deviation, if the excess of density were twice as great as the deficiency, a deviation of $5''$ would correspond to an acceleration of half a second; if 3 times as great, to $\frac{3}{4}$; if 4 times, to $\frac{2}{5}$; and if five, to a second.

It may perhaps be considered as an omission in this calculation, that the attraction of the parts of the earth's surface, elevated by means of the irregular gravitation, has not been included in it. But it depends on the supposition that we may adopt respecting the cause and date of the irregularity, whether or no we ought to consider it as likely to have occasioned such a general elevation; and it does not appear that the result of the computation would very materially alter our conclusions, though it would be somewhat laborious to go

through all its steps with precision. It would indeed be so much the more superfluous to insist on this minute accuracy, as variations so much more considerable in the form of the earth's surface are commonly neglected: for example, in the allowance made for the reduction of different heights to the level of the sea, which has usually been done without any consideration of the attraction of the elevated parts, interposed between the general surface and the place of observation. It is however obvious, that if we were raised on a sphere of earth a mile in diameter, its attraction would be about $\frac{1}{8000}$ of that of the whole globe, and instead of a reduction of $\frac{1}{2000}$ in the force of gravity, we should obtain only $\frac{3}{8000}$, or three fourths as much: nor is it at all probable that the attraction of any hill a mile in height would be so little as this, even supposing its density to be only two thirds of the mean density, of the earth: that of a hemispherical hill would be more than half as much more, or in the proportion of 1.586 to 1; and it may easily be shown, that the attraction of a large tract of table land considered as an extensive flat surface, a mile in thickness, would be three times as great as that of a sphere a mile in diameter: or about twice as great as that of such a sphere of the mean density of the earth: so that, for a place so situated, the allowance for elevation would be reduced to one half: and in almost any country that could be chosen for the experiment, it must remain less than three fourths of the whole correction, deduced immediately from the duplicate proportion of the distances from the earth's centre. Supposing the mean density of the earth 5.5, and that of the surface 2.5 only, the correction, for a tract of table land, will be reduced to $1 - \frac{3}{4}$.

$$\frac{2.5}{5.5} = \frac{29}{44}, \text{ or } \frac{66}{100} \text{ of the whole.}$$

4. EULER's formula for the rolling pendulum.

I beg leave to observe, in conclusion, with regard to Mr. LAPLACE's theorem for the length of the convertible pendulum rolling on equal cylinders, that its perfect accuracy may readily be inferred, without any limitation of the form of the pendulum, or of the magnitude of the cylinders, from the general and elegant investigation of EULER, which also affords us the proper correction for the arc of vibration. This admirable mathematician has demonstrated, in the sixth volume of the *Nova Acta Petropolitana*, for 1788, p. 145, that if we put k for the radius of gyration with respect to the centre of gravity, a for the distance of the centre of gravity from the centre of the cylinder, c for the radius of the cylinder, h^2 for $k^2 + (a - c)^2$, and b for the sine of half of any very small arc of semivibration, we shall have, for the time of a complete oscillation, $\frac{\pi b}{\sqrt{(2ag)}} + \frac{\pi b b (b b + 4ac)}{4b \sqrt{(2ag)}}$, and ultimately, if $b = 0$, $\frac{\pi b}{\sqrt{(2ag)}}$ only, which, for a simple pendulum, of the length a , k and c both vanishing, becomes $\frac{\pi \sqrt{a}}{\sqrt{(2g)}}$; and for any other length l , $\frac{\pi \sqrt{l}}{\sqrt{(2g)}}$; consequently, making $\frac{\pi \sqrt{l}}{\sqrt{(2g)}} = \frac{\pi b}{\sqrt{(2ag)}}$, we have $\sqrt{l} = \frac{b}{\sqrt{a}}$, and $al = hh = k^2 + a^2 - 2ac + c^2$. Now if we find another value of a , which will fulfil the conditions of the equation, all the other quantities concerned remaining unaltered, and add the two values together, we shall have the distance of the centres of the two cylinders corresponding to the length l of the equivalent pendulum; but since $a^2 - (l + 2c)a = -k^2 - c^2$, we have $a - \frac{1}{2}l - c = \pm \sqrt{\dots}$, and $a = \frac{1}{2}l + c \pm \sqrt{\dots}$, so that the sum of the two values of

a must be $l + 2c$, that is, the distance of the centres of the cylinders must exceed the length l by twice the radius, and l must be precisely equal to the distance of their surfaces.

Believe me, dear Sir,

very sincerely your's,

THOMAS YOUNG.

Welbeck Street, 29 Dec. 1818.

VI. *On the anomaly in the variation of the magnetic needle as observed on ship-board.* By William Scoresby, jun. Esq. Communicated by the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S.

Read February 4, 1819.

THE anomalies discovered in magnetical observations conducted on ship-board, were usually attributed to the imperfection of the azimuth compass, until Captain FLINDERS, in his modest and enlightened paper on this subject, published in the Philosophical Transactions, suggested that they were probably owing to the concentration of the magnetic influence of the iron, made use of in the construction of the ship. The truth of this suggestion, and the accuracy of his observations, have since met with full confirmation, and his practical rules founded thereon have received additional support, from the "Essay" of Mr. BAIN "on the variation of the Compass," published last year.

As I have been materially anticipated by Mr. BAIN, in a series of observations on the variation of the compass,* which

* The azimuths contained in the following table were taken, either by the needle of a theodolite, or by a compass fitted up at sea, for the purpose, with a card made extremely light, and a bar fastened edgewise to it, by two brass screws, *a a*, as in the annexed sketch. The compass being small, the card light, and the needle very powerful, owing to the thickness of its ends, it performed considerably better than an expensive azimuth compass of larger dimensions, which indeed was so sluggish and erroneous in its indications, that I could make no good use of it.



I conducted on the coast of Spitzbergen, in the years 1815 and 1817, it will be unnecessary here to enter into the detail of these observations or enlarge upon the probable cause of the anomalies observed ; it may be sufficient to give a table of the most accurate of my observations, and annex to it the few general inferences which were drawn from it, during the voyage in which most of the observations were made, together with such remarks on each inference as seemed to me calculated for its elucidation. I shall, however, just premise, that I am not unconscious of the great liability to error in observations of this kind, and of the variety of causes (arising out of the unequal distribution of iron in different ships, whereby numerous local attractions are formed) which contribute to the multiplication of those errors : it is, therefore, with the greatest deference that I submit these deductions, particularly as I conceive it will require observations to be made under a vast variety of circumstances, and in many different vessels, before *correct* and satisfactory conclusions can be drawn. It is *only* then as a step towards facilitating such general conclusions, the importance of which to our maritime concerns is so obvious, that I presume to offer these observations and remarks.

Table of Magnetical Observations, made on board of the ship Esb, in voyages to the Greenland or Spitzbergen Whale Fishery; with a view to investigate the laws by which the anomalies, or errors, discovered in the indications of the Mariner's Compass on ship-board, are regulated.

No.	Date.	Latitude.	Longitude.	Time. [Ch. Chrono- meter. [Ap. Appa- rent.]	Sun's Azimuth.		Variation.		Errors attributed to the position of the ship's head. [Diff. of head. [From col. VIII. & IX.]]		Ship's head by binnacle compass.	Situation of the compass by which the Sun's azimuth was observed. [Col. VI.]	
					Observed.	True.	Apparent or observed.	Supposed true.	Differences produced by a change in position of the ship's head. [From col. VIII. & IX.]]	Differences produced by a change in position of the ship's compass. [From col. VIII. & IX.]]			
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.	
1	June 30	77° 19' N.	12° 15' E.	Ap. Noon.	S. 7° W. 38	South.	7° W. 38	21° W. 21	14° 0' 17 0	-	31° 0'	S. by E.	Starboard side of the main deck.
2	-	-	-	-	-	South.	-	-	0 0	-	17 0	S. by E.	Larboard ditto.
3	1817.	-	-	-	21	South.	21	21	0 0	-	-	S. by E.	Centre of main hatches.
4	June 6	77 56	0 15 W.	Cb. 6h 54 P.M.	N. 36 W. 35½	N. 73° W. 64 30	37 28.45	32 30	4 30	8° 15'	-	W. ½ S.	On the binnacle amidships of the quarter-deck, 10 ft. from taffrail.
5	-	-	-	7 28	35½	64 30	37	32 30	4 30	-	-	East.	Compass on a stand on the middle of the quarter-deck, 6 feet abaft capstern spindle.
6	-	-	-	7 28	45	64 30	19.30	32.30	13 0	-	9 15	W. ½ S.	Starboard side of the main-deck.
7	-	-	-	6 54	27½	73 0	45.30	32.30	13 0	26 0	-	W. ½ S.	Larboard side of the main-deck.
8	-	-	-	7 32	33	63 30	30.30	32.30	2 0	-	-	E. ½ S.	On the binnacle, 10 ft. from taffrail.
9	June 19	77 36	2 35 W.	7 33	31	63 15	32.15	32.30	0 15	-	1 45	E. ½ S.	Ditto.
10	-	-	-	Ap. 11 38	25	S. 5 30 E.	30.30	36.0	5 30	-	-	E. by S.	Starboard side of the main-deck.
11	-	-	-	11 20	27	10 0	37.0	36.0	1 0	6 30	-	N.	Ditto.
12	-	-	-	12 3	34½	S. 0 45 W.	34.0	36.0	2 0	3 0	-	N. ½ E.	Ditto.
13	-	76 24	4 45 W.	Cb. 7 34	N. 46 W.	N. 68 11 W.	22.11	37.0	14 49	-	-	NNE.	Starboard side of the main-deck.
14	-	-	-	7 38	16½	67 11	50.41	37.0	13 41	-	28 30	NNE.	Larboard ditto.
15	July 16	76 6	0 30 W.	Cb. 6 33	60	79 54	19.54	33.0	13 6	-	-	South.	Starboard side of the main-deck.
16	-	-	-	6 36	33	79 9	46.9	33.0	13 9	-	26 15	South.	Larboard ditto.
17	-	-	-	6 31	46	80 24	34.24	33.0	1 24	-	11 45	South.	Centre of main-hatches.
18	-	-	-	6 44	53	77 9	24.9	33.0	.8 51	-	-	South.	Centre of fore-hatches.
19	-	-	-	6 43	41	77 24	36.24	33.0	3 24	-	12 15	South.	Larboard side fore-castle near the windlass end.
20	-	-	-	6 41	70	77 54	7.54	33.0	25 6	-	28 30	South	Starboard ditto.

From these observations, and from the assistance afforded by the lucid remarks of Captain FLINDERS, the inferences which follow are deduced.

1. In the construction of every ship, a large quantity of iron being used, the portions thereof which have a perpendicular position, such as standard and hanging knees, the nails and bolts in the deck, the capstern spindle, flukes of the anchors (when at sea), chain-plates, iron stanchions and riders; the eye bolts, transom bolts, joint bolts, &c. of gun carriages, and possibly the upper surfaces of the guns themselves, &c. &c. have a tendency to become magnetical, the upper ends being *south* poles and the lower *north* poles, in this hemisphere, where the *north* end of the needle dips, but the contrary in the southern hemisphere, where the *south* end of the needle dips.

2. The combined influence of the iron distributed through all parts of the ship, seems to be concentrated into a kind of magnetic *focus of attraction*, the principal south pole of which being upward in the northern hemisphere, is probably situated, in general, near the middle of the upper deck, but nearer to the stem than the stern.

Wrought iron having a much greater attraction for the magnetic needle than cast iron, the anchors, which usually lie about the bows, possess much more influence than guns; hence, the focus of attraction lies nearer to the bows than to the stern.

3. This focus of attraction so influences the compass needle, that it is subject to an *anomaly*, or variation from the true meridian, different from what is observed by a compass on shore; the north point of the compass being constantly drawn towards the focus of attraction, which appears to be a

south pole in north dip; and the south point being attracted in south dip, where the focus of attraction probably becomes a north pole.

The phenomenon of a ship appearing to lie nearer the wind when beating to the northward, with the wind at north, than when beating to the southward, with a southerly wind, was observed by my father at least 20 years ago, which phenomenon he attributed to the "attraction of the ship upon the compass;" and ever since the year 1805, I have been in the habit of allowing only 2 to $2\frac{1}{4}$ points variation on the passage outward to Greenland, with a northerly or northeasterly course, but generally 3 points variation on the homeward passage when the course steered was S. W. or S. W. b. W. Without this difference of allowance, a Greenland ship outward bound will be generally found to be to the eastward of the reckoning, and homeward bound will be even 4 or 5 degrees to the eastward of it.

4. This anomaly in the variation of the compass, occasioned by the attraction of the iron in the ship, is liable to change with every alteration in the dip of the needle, in the position of the compass, or in the direction of the ship's head.

If the intensity of the terrestrial magnetism be not equal in all parts of the globe, then the anomaly in the variation of the compass will be also liable to change with every alteration in the magnetic influence of the earth. This is a point of such importance, I conceive, in the science of magnetism, that I was very anxious to procure a dipping needle on my last voyage to Greenland, to ascertain whether the magnetism of the earth, by which the dipping needle is influenced, be not greater near the magnetic pole, than it is in England.

If it be equal, the oscillations of the same dipping needle would be performed, circumstances as to temperature and "local attraction" being the same, in equal spaces of time in both places; but if the magnetic power in either place be greater, the oscillations of the needle would there be quicker. The number of vibrations of a horizontal needle, performed in a certain space of time in Greenland, is to the number performed in an equal space of time in England as 5 to 6, each longer vibration in England being performed in 5 seconds, and in Greenland in 6. No alteration was observed in the time required for each vibration, whether the temperature was high or low, but I think in a low temperature the vibrations performed by the needle before it stopped were fewer.

5. The anomaly of variation bears a certain proportion to the dip of the needle, being greatest where the dip is greatest, diminishing as the dip decreases, and disappearing altogether on the magnetic equator.

Captain FLINDERS ascertained, that the medium error or anomaly for 8 points deviation of the Investigator's head, on either side of the magnetic meridian, was very nearly $\frac{1}{20}$ of the dip, .05 the decimal expression of which, he considered to be the common multiplier to the dip, for obtaining the radius of error at any situation in the southern hemisphere; and .053 to be the common multiplier, from England to the magnetic equator. This, however, can only be correct within certain limits, as on the magnetic pole, where the anomaly would probably be equal to the dip, or 90° , the decimal multiplier would require to be increased to 1.0. Hence it has been suggested, by an officer on board one of the vessels now in search of a north-west passage, that in those parts of the globe where the dip is 90° , the compass needle would pro-

bably always stand N. and S., by the attraction of the ship. This position clearly follows from the inference above, provided the compass be placed near the ship's stern in midships; but if placed as described in inference No. 8, the ship's head by the compass on the starboard side of the main deck, would always appear to be *east*, and on the larboard side *west*.

6. A compass placed near the stern, amidships of the quarter-deck, is subject to the greatest anomaly or deflection from the magnetical meridian, when the ship's course is about west or east; because the focus of attraction then operates at right angles to the position of the compass needle; but the anomaly disappears when the course is about north or south, because the focus of attraction is then in a line with, or parallel to, the compass needle, and consequently has no power to deflect it from its direct position. [See Observations, No. 4, 10, 11, and 12 of the prefixed table.]

This situation for the *binnacle* is deemed one of the best in the ship, and is very properly preferred. Being abaft the focus of attraction, the north point of the compass, in this magnetic hemisphere, is always attracted forward, and the errors at equal distances from the magnetic meridian, in the same dip, are alike in quantity both on easterly and westerly courses, and always towards the north; the correction, when applied to the apparent course, must therefore be towards the south, to give the true course steered. Thus in high northern latitudes, where the anomaly is great, (say 20° , or 10 degrees on each side of the magnetic meridian) a ship steering west by the compass 100 leagues, and then east 100 leagues, instead of coming to the place from whence she started, will be 104 miles to the southward of it.

7. The greatest anomaly with the compass in the position

last described, being ascertained by observation, the error on every other point of the compass may be easily calculated; the anomalies produced by the attraction of the iron in the ship, being found to be proportionate to the sines of the angles between the ship's head and the magnetic meridian.

Captain FLINDERS's rule is—As the sine of eight points (or radius) is to the sine of the angle between the ship's head and the magnetic meridian (or sine of the course reckoned from south or north) so is the anomaly found at east or west by observation, to the anomaly on the course steered; or, the anomaly on any other course being found by observation, the error on that position of the ship's head “ would be to the error at east or west, at the same dip, as the sine of the angle between the ship's head and the magnetic meridian, to the sine of eight points, or radius.”

8. A compass placed on either side of the ship's deck, directly opposite to, or abreast of, the focus of attraction, gives a correct indication on an east or west course, but is subject to the greatest anomaly when the ship's head is north or south; and being here nearer the focus of attraction, the anomaly is much greater than that observed on an east or west course with the compass placed in the binnacle near the ship's stern.

This inference is founded on observations, No. 1, 2, 3, 8, 9, 13, 14, 15, 16, and 17, of the prefixed table. The latter part of the inference, namely, that the greatest anomaly occurs here when the ship's head is north or south, is fully and uniformly established; but the former part rests only on the authority of observations No. 8 and 9, though it derived

additional support from several observations which I have excluded, because neither the sun, nor any other distant object, calculated for proving the accuracy of the observations and determining the clear effect of the "local attraction," was visible.

9. A compass placed within six or eight feet of a capstern spindle, or anchor, or other large mass of wrought iron, foregoes, in a great measure, the influence of the focus of attraction, and submits to that of the nearer body of iron.

The effect of this is various, according to the relative position of the compass and the iron. When the compass is placed directly *abaft* the body of iron, the influence is similar to, but greater than, that of the focus of attraction on a compass placed near the stern, as described in inference No. 6. [See Table of observations prefixed, No. 6 and 7.] When placed directly *before* it, the anomaly is similar in quantity, but has its sign reversed; and when placed on either side of the mass of iron, the influence corresponds more nearly with that of the focus of attraction on a compass placed in the sides of the ship opposite to it, as described in inference No. 8. A compass placed upon the *drum head* of the capstern, any where out of the centre, will have its north point so forcibly attracted by the upper end or south pole of the spindle, that the ship's head may be made to appear to be directed to any point whatever, at the pleasure of the experimenter. I have sometimes excited the astonishment of my officers by taking the binnacle compass and so placing it on the capstern head, that the ship has appeared to be steering a course directly contrary to that intended.

10. When the iron in a ship is pretty equally distributed

throughout both sides, so that the focus of attraction occurs in midships, a compass placed on the midship line of the deck (drawn longitudinally) will be free from any anomaly from one end of the ship to the other, when the course is north or south; but on every other course an anomaly will generally appear, increasing as the angle between the ship's head and the magnetic meridian increases, until the error is at a maximum, when the course is east or west.

The unequal distribution of iron in the ship, on board of which I made all my experiments, prevented the above effect from being realized. A blacksmith's shop was situated between decks, on the larboard side of the fore hatchway. It was lined with sheet iron, and besides the armourer's forge, vice, &c. contained a large quantity of other iron. The effect of this, together with the anchors, windlass necks, and other iron, was very remarkable on a compass placed in different parts of the deck near the foremast. [See Observations, 18, 19, and 20 of the prefixed table.]

11. As a compass placed on the midship line of the deck is subject to no anomaly fore and aft, in certain ships, on a north or south course [Inference No. 10], and as a compass in either side of the ship, opposite to the focus of attraction, shows no anomaly on a west or east course [Inference No. 8], the intersection of the line joining the two situations in opposite sides of the ship with the midship line traced fore and aft, will probably point out a situation directly over the top of the focus of attraction, where no anomaly on any course whatever will appear.

The *Esk*, in which I made my magnetical observations, had, as above stated, an armourer's forge near the larboard bow,

which with the varying position of large quantities of iron work, composing our whale fishing apparatus, contributed to vary this point where no anomaly is supposed to exist, and prevented me from ascertaining satisfactorily, at any time, its precise situation. I made indeed but very few observations with this view, and these I find neither establish nor refute the inference.

12. The anomaly of variation is probably the greatest in men of war, and in ships which contain large quantities of iron; but it exists in a very considerable degree also in merchantmen, where iron forms no part of the cargo, especially in high latitudes, where the dip of the needle is great.

WILLIAM SCORESBY, Jun.

Whitby, 3d November, 1818.

VII. *On the genus Ocythoë; being an extract of a letter from Thomas Say, Esq. of Philadelphia, to Wm. Elford Leach, M. D. F. R. S.*

Read February 4, 1819.

I HAVE before me a specimen of *ocythoë* in an *argonauta*, forming part of the collection of the Acad. of Nat. Sciences. It was taken from the stomach of a dolphin, which was caught in soundings on our Atlantic coast, and is in the most perfect state of preservation, not having suffered the slightest decomposition from gastric action.

It is sufficiently distinct from your *O. Cranchii*, as well as from the animal of *nautilus sulcatus* of KLEIN; and if the figure given by SHAW of the animal of *argonauta argo* has any pretensions to accuracy, it is most probably an unknown species.

I here attempt a description of it, and also submit a few remarks on the genus.

Ocythoë punctata.

Body pale, punctured with purplish; abdomen conic-compressed, vertical, semifasciate near the summit, with a profoundly indented transverse line; arms much longer than the body, attenuated, filiform at their tips, alated; membranes rounded.

Inhabits the Atlantic ocean near the North American coast.

Descrip. *Abdomen* conical, slightly compressed, nearly vertical with respect to the disk of the head, with a profoundly indented transverse line, which extends half round, near the summit. *Arms* attenuated, much longer than the body, filiform towards the tip, slightly varied with brassy,

With respect to the contested question relative to the parasitic nature of the animals of this genus, I believe the remark will hold good generally, if not absolutely, that those mulluscous animals that form the shell in which they reside, are more or less connected with it by muscular or membranaceous attachment, or by the permanent spiral form of the posterior part of the body; and that the body of the animal complies with the inequalities of the chamber of the shell, or rather that the shell is moulded upon the body, so as to be in contact with it in every part. So careful are they to fill the cavity to its very summit, that when from their increase of growth the apex of the shell is vacated in consequence of its straightness, either that part is removed by the animal, and additional calcareous matter is secreted to close the aperture thus formed, or it is permitted to remain, and the cavity is filled up by the same secretion; of the former process we have an instance in *Bulimus decollata*; and of the latter many instances occur, familiar to the knowledge of conchologists. The *Ocythoë* offers to our consideration a remote deviation from these ordinary laws which apply to the testaceous mollusca, inasmuch as it only resides in the last volution or body of the shell. In the specimen above described, the sides of the abdomen are slightly canaliculated, in conformity with the sculpture of the inner lateral surface of the shell; but it is worthy of remark, that the portion which corresponds with the most unequal part of the chamber, the carina, is not at all indented; which fact induces the supposition that the shell does not fit the body, and of course was not made for it, otherwise it does not seem probable that the body would

be remote from the shell in one part, and impressed by its asperities in another.

Such also is the form of the inferior part of the abdomen, that it never could have revolved in the cavity of the involuted spire; yet we have never been informed that the vacated spire has been either broken or solidified. Neither is there any attachment whatever between any part of the body and the including shell, by an organ appropriated to that office. In consequence of this organization, the *ocythoë* cannot adapt itself to the form of the cavity in which it rests, or secure itself there so completely as the well known parasitic paguri are enabled to do, in consequence of the pliability of their vesicular abdomen, and by the agency of their terminal hooks or holders. Such observations seem to afford presumptive evidence of the parasitic nature of these animals.

It does not appear to me probable that the *Ocythoë* ascends to the surface of the water by exhausting its shell of the included water; for if this were the fact, those females whose shell is in great part filled with eggs, could not visit the surface. But the change of specific gravity is doubtless effected in its own body, by which it is enabled to sustain itself on the surface at will, or to descend to the bottom promptly at the approach of danger.

The shells which in structure and appearance approach nearest to *argonauta*, are unquestionably to be found in the PTEROPODA; and the examination of *Carinaria*, *Atlanta* and *Spiratella*, would almost lead us to suppose, that the artificer of *argonauta* is in reality of that division; but if this supposition be indicated by the conformation of the shell, it does

not seem to be corroborated by the probable habits of the animal. All those hitherto discovered of that group, are known to swim at the surface of the ocean, and not being furnished with other organs of locomotion than fins, they cannot glide upon the bottom; we must therefore (analogically) suppose this to have been the habit of the animal; and yet it is hardly admissible that it should, in that case, have eluded the observation of voyagers, since the shell has not unfrequently been found in a state of occupancy by the parasite.

VIII. *On Irregularities observed in the direction of the Compass Needles of H. M. S. Isabella and Alexander, in their late Voyage of Discovery, and caused by the attraction of the iron contained in the Ships. By Captain Edward Sabine, of the Royal Regiment of Artillery, F. R. S. &c.*

Read February 18, 1819.

IT is proposed in this paper to show in what respects the effects of local attraction, in the above mentioned ships, were conformable to the observations which had been made in preceding voyages; and how far the errors, which were found to take place on different courses, and under different dips of the magnetic needle, corresponded with the rules for calculating corrections, which Captain FLINDERS had found useful in his own experience, and which he had recommended for a more extensive trial.

It may be desirable to premise, that the irregularities here alluded to, are not those accidental disturbances which may be caused by iron placed inadvertently too near the compasses; but the permanent, and constant effect of the mass of iron contained in a ship, affecting its compasses at all times, and in a greater or less degree, according as its influence is more or less powerful, in comparison to the directive force of the earth's magnetism. That errors have always existed from this cause, may be inferred, from the uncertainty which experience has attached to the results of azimuths observed in ships. The cause, however, appears to have been very long

unsuspected, whilst its effects had produced a general impression, that the azimuth compass was in itself an imperfect instrument, and only to be relied on within certain undefined and variable limits.

It was reserved to the accurate observation, and the habit of recording and comparing apparently trivial and accidental differences in results, which distinguished the late Mr. WALES (astronomer in the second voyage of Captain Cook,) to enable him to lead the way to a knowledge of the nature and causes of these errors; he remarks, "that in the passage of the Resolution and Adventure to the Cape of Good Hope, and subsequently, the greatest west variations had happened when the ship's head was north and easterly, and the least when it was south and westerly, differing very materially from one another with the ship's head in different positions, and still more when observed in different ships;" thus manifesting that they were something more than accidental.

This voyage was the last in which Mr. WALES embarked, and the investigation does not appear to have been pursued in this country until the voyage of discovery to Terra Australis, in the first years of the present century. The survey of the coast of New Holland being carried on in a considerable measure, by the intersection of compass bearings taken from the deck of the Investigator, so much embarrassment and perplexity were found to arise from the effects of local attraction, that much of Captain FLINDERS's attention and thoughts were necessarily devoted, to a consideration of some means of remedying the inconvenience.

On his return to England, he obtained permission from the Lords Commissioners of the Admiralty to make a course of

experiments in ships under their direction at the principal sea ports, with a view to ascertain if compasses were similarly affected in other ships, and to try the general applicability of rules which he had found useful in correcting the errors in the Investigator. These rules, with the observations and reasonings on which they were founded, were published in a short paper in the Philosophical Transactions, and in a more detailed form in Appendix No. II. in the Voyage to Terra Australis. There are three points in these statements chiefly worthy of attention, from their practical importance; and on which it seems desirable, therefore, to notice how far his observations have been confirmed by those made in the Isabella and Alexander.

FIRST; he found that in every ship a compass would differ very materially from itself, on being removed from one part of the ship to another. Experience of this source of irregularity, had induced him early in his voyage, to confine the use of the compass, with which his survey was carried on, to one particular spot. The place he selected was determined by convenience in other respects; it was on the binnacle, and exactly amidships.

The Isabella and Alexander had not completed half their voyage across the Atlantic, before it was found that the binnacle compasses of the one ship differed very materially, in indicating the course steered, from those of the other: namely, one point, or $11\frac{1}{4}^{\circ}$. No dependance whatsoever could be placed on the agreement of compasses in different parts of the ship, or of the same compass with *itself*, if removed but a few inches: even in the neighbourhood of the binnacles the variation, as observed amidships, was from 8° to 10°

greater than the result of azimuths taken by a compass placed between two or three feet on the larboard side; and an almost equal difference in a contrary direction took place on removing the compass to the starboard side, rendering it a matter of some trouble and difficulty, to make the azimuth compass agree with those in the binnacle by which the ship was steered, and for which it was therefore necessary to determine the variation.

As the ships ascended Davis's Strait, these latter compasses began to traverse so sluggishly, that it was necessary to shake the binnacles continually to assist their motion. The cards of these* had a metal rim round their circumference, weighing one ounce, eleven drams avoirdupois, which, as the directive power of magnetism diminished, became too heavy for the needle to carry round: they were also frequently found to disagree with each other from $\frac{1}{4}$ or $\frac{3}{4}$ of a point; the consequence, most probably, of the different local attraction to which they were exposed. These compasses ceased therefore to be attended to, except as an occasional assistance to the helmsman, and a position was selected in each ship, in which a compass on a more suitable construction was permanently fixed; by this the ship's course was directed, azimuths taken, and bearings of land, &c. noted, during the voyage.

This *standard compass*, as it may be called, was placed in the *Isabella* exactly amidships, between the main and mizen mast, on a stout cross beam elevated nine or ten feet above the deck; this beam was the usual walk of the Greenland pilot, or of the quarter master, as affording a better view of the ice among which the ships were frequently steered, than

from the deck. The elevation was an advantage to the compass in such high magnetic latitudes, by rendering it less liable to accidental disturbance, on the removal of such implements of iron as were required to be kept on deck for use. The *Alexander* not having a similar cross beam, her compass was fixed amidships on a box of sand placed on the companion, between five and six feet above the deck.

SECONDLY; Captain FLINDERS found that in his compass permanently fixed as described, no error took place when the ship's head was on the magnetic north or south points; showing, that at such times the attraction of the ship, and of magnetism, were in the same line of direction. The maximum of error also took place when the ship's head was at right-angles to these points; namely, at east or west; being however in opposite directions, in excess of the true variation on the one side, and in defect on the other; so that the extreme difference occasioned by altering the course from east to west or the reverse, would be twice the error at either.

On the intermediate points, the ratio of the error to its maximum was as the "sine of the angle between the ship's head and the magnetic meridian to the sine of eight points or radius," or sufficiently near to admit of corrections being calculated for every course, when the error on a single one was known by observation.

Thus far the experiments which Captain FLINDERS tried in every ship corresponded, excepting only that the maximum of error in different ships at the same place would differ materially.

The accordance in so many ships gave him reason to believe that in compasses placed near the binnacle, and amidships, *the points of no error* would be most commonly

those of the magnetic meridian. Considering, however, that this must depend altogether on the distribution of iron, and may be therefore liable to great diversity, he recommends, that, in every ship, as soon as a fixed position has been selected for a compass, the points of no error should be determined by repeated observation. The method that was adopted for this purpose in the late voyage appearing both simple and effectual, it may be useful to exemplify it by an instance or two.

The *Isabella* being at anchor in Brassa Sound, Shetland, her head was placed, by means of warps, on each point of the compass successively, and the bearing of a pile of stones on the summit of a distant hill noted by her compass at each point; at the same time that these observations were made on board, her bearing from the hill was also observed by a compass placed on the pile of stones; the *agreement* in bearing showed the points of no error, and the *differences* the errors in each point, without the calculation which azimuths involve. [See Pl. X. fig. 1.]

The *Alexander* being along side a floe of ice in Baffin's Bay, the true magnetic bearing from the ship, of a very distant and well defined object on the main land, was found by carrying a compass on the ice in an opposite direction, to a distance which insured its being perfectly free from local influence. The ship's head being then warped round to each point of the compass successively, the errors in each were determined by the difference in bearing, as in the last instance. [See Pl. X. fig. 2.]

The regularity in the above results is the best testimony that the method is a satisfactory one. Certain precautions must be attended to: thus, the object must be sufficiently

distant, that the space occupied in warping the ship round may not subtend any sensible parallax. The direction of the ship's head should be noted by the compass by which the bearings are taken. A short time must be allowed to elapse after the ship is steady on any point, to ensure the traversing of the cards: this is particularly necessary in high latitudes when the compasses move very sluggishly. And lastly, the observations should be repeated.

It will be observed by the above results, in the *Isabella* and *Alexander*, that the points of no error were not coincident in either ship with those of the magnetic meridian; in the *Alexander* especially, they were more nearly at right angles to it. That this ship should have differed so materially from all the instances on record, may be attributed to her compass being so near the level of the deck, and therefore being more affected by the influence of a considerable quantity of iron articles (such as ice anchors, ice saws, &c.) which were carried on the after part of the deck for convenience in use, than it would have been, had it been raised higher. This was proved by placing a compass on a plank elevated for experiment in front of the companion, to the same height as in the *Isabella*, namely, nine or ten feet. The points of no error were found, in this position, to be about north and south, and the amount of error at eight points, nearly twenty degrees; the same as in the *Isabella*: the greatest error at the same time, by the *Alexander's* standard compass, viz. the one nearer the decks, being $8^{\circ} 20'$ at N. N. E. The dip was $84^{\circ} 09'$.

The propriety of Captain FLINDERS's recommendation, to determine the points of no error in a fixed compass by actual

observation in every ship as soon as the distribution of iron is completed, may therefore be considered as confirmed by the observations in the *Isabella* and *Alexander*; whilst his rule of proportion may receive a verbal alteration to render it more suitable for general application: so corrected, it would be as follows. The expressions substituted being marked in italics, and the original words entered in the margin.

	“ The error produced in any direction of the
	ship’s head, will be to the error at <i>the point of</i>
East or west.	<i>the greatest irregularity</i> , as the sine of the angle
	between the ship’s head and <i>the points of no</i>
Magnetic meridian.	<i>error</i> to the sine of eight points or radius.”

THIRDLY; Capt. FLINDERS’s experience in the Investigator showed that the maximum of error in the same compass, would be different in different parts of the world, although the use of the compass was confined to one particular spot in the ship, and every precaution taken to avoid an interference with the distribution of the ship’s iron.

It is worthy of remark, that by multiplying observations and by comparing the series one with another, he was thus *practically* led to trace a connection between the amount of the errors and the dip of the needle; a knowledge of the fact preceding, in his mind, any theoretic suggestion that such might be the case.

It does not appear indeed that the principal cause of this connection was even subsequently known to him; he perceived that the influence of local attraction on the compass needle increased as the dip became greater. He endeavoured to account for this circumstance, on a supposition that all iron

might receive an *absolute* increase in the intensity of its attractive power by approaching the magnetic pole.

The increase, however, which was the subject of his observation, was a *relative* one, being in comparison to the directive power of magnetism. A diminution in the latter would therefore produce the effect equally with an absolute augmentation in the former; and that such a diminution does take place, and in a degree which is sufficient to account for all the effects observed, will be evident to every person, who reflects that although the magnetic force is greatest at the pole, its directive power must then have wholly ceased, having become less on the horizontal traversing of the needle in proportion as the point of attraction has been brought beneath the compass; indicated by the angle which the dipping needle makes with the horizon. This is doubtless the principal cause of the connection which Captain FLINDERS was the first to trace.

It is not designed to say that this cause may not be aided by the increased magnetism of portions of the ship's iron, such as bars and stanchions; which being fixed in an upright position, may receive an addition to their attractive power where the position of the dipping needle is always coincident with theirs; but merely to observe, that a cause is known to exist for the connection, independently of supposition; which cause, conjointly with experience, shows the inadequacy of the rule proposed by Captain FLINDERS, whereby the amount of error, under any known dip, being ascertained, the amount may be calculated for any other dip, by using as a multiplier, the decimal expression of the proportion which

the error, in the one ascertained instance, may have borne to the dip.

In the observations made in the *Isabella* at Shetland, where the dip is $74^{\circ} 21\frac{1}{2}'$, the maximum of error was $5^{\circ} 34'$ easterly of the true variation, with the ship's head at E. S. E. and $5^{\circ} 46'$ westerly at W. N. W. making an extreme difference of $11^{\circ} 20'$.

By Captain FLINDERS's rule, the common multiplier for this compass would have been about one twelfth, or .083, which at a dip of $86^{\circ} 09'$, which was the greatest observed during the late-voyage, would have given an error of between 7° and 8° , making the extreme difference 15° ; whereas repeated observation showed it to be at that time more nearly 50° , if not exceeding that amount.

The inadequacy of the rule will also appear by reference to the observations made by the *Alexander* in Baffin's Bay. The error at eight points being $6^{\circ} 46'$, at a dip of $84^{\circ} 30'$; it ought scarcely to have exceeded 7° at the greatest possible dip, making an extreme difference of less than 15° . No opportunity occurred indeed of making accurate observations at a greater dip than the above; but the difference in the bearing of objects before and after tacking indicated with sufficient certainty, that the error had increased to an amount very far beyond 15° ; frequent instances of an extreme difference of from 3 to 4 points being remarked, as the ship approached the farthest western longitude to which she attained in a high latitude; this was in Lancaster's Sound of Baffin, into which inlet the expedition sailed beyond the 81° of west longitude in the parallel of 74° and a few minutes.

It is much to be regretted that the service did not admit an opportunity to be afforded, of making observations on the

various magnetic phenomena, with the excellent instruments supplied to the expedition, at this very interesting place; where a nearer approach was made to one of the magnetic poles than had ever been known before.

But in the absence of any actual observation on the dip of the needle, this fact of the error of the compasses having increased from local attraction so greatly beyond the amount which had been before observed, is worthy of notice, as affording an indication that the dip had also increased, and not inconsiderably. The greatest which was observed, was $86^{\circ} 09'$; and after this observation, the ships continued to sail for six days in the direction in which the dip had hitherto been found to increase.

In concluding this paper, it may be permitted to remark, that it is to the voyages of discovery undertaken during the reign of his present Majesty, that a knowledge of the extent and causes of the errors to which a compass is subject in ships, is to be principally attributed; as well as the steps that have been taken towards the investigation and remedy of the inconvenience they occasion to practical navigation.

The care and exertions of Captain FLINDERS in collecting observations for this purpose, give his opinions and rules a peculiar claim to attentive consideration. No one could have been more fully persuaded than he was, that a rule, founded on the effects experienced in a few ships, would require a far more extensive trial, before it could be depended on for general application.

To carry this on, therefore, is to follow his useful example, and to effect what he was desirous to have done himself, had his life been spared.

Fig. 1.

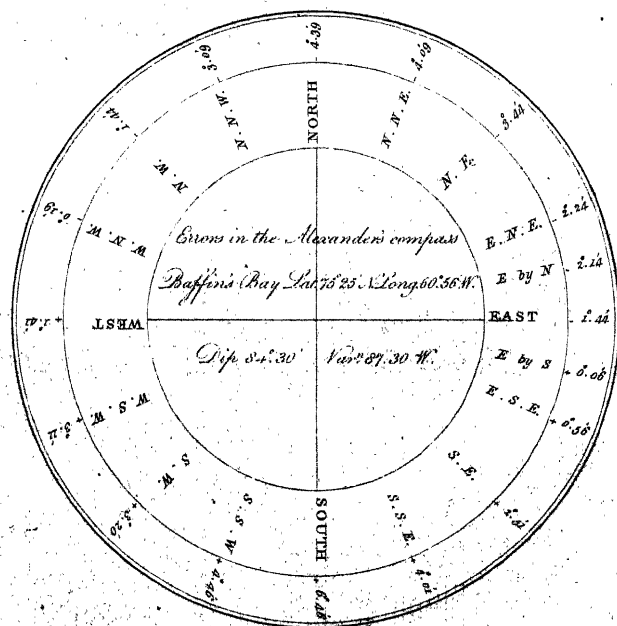
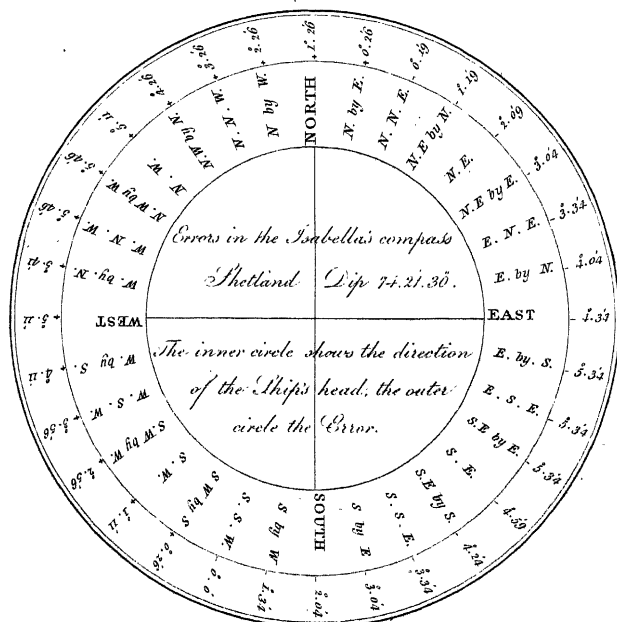


Fig. 2.

IX. *Some observations on the formation of Mists in particular situations.* By Sir H. Davy, Bart. F. R. S. V. P. R. I.

Read February 25, 1819.

ALL persons who have been accustomed to the observation of Nature, must have frequently witnessed the formation of mists over the beds of rivers and lakes in calm and clear weather after sun set; and whoever has considered these phenomena in relation to the radiation and communication of heat and nature of vapour, since the publication of the researches of M. M. RUMFORD, LESLIE, DALTON, and WELLS, can hardly have failed to discover the true cause of them. As, however, I am not aware that any work has yet been published in which this cause is fully discussed, and as it involves rather complicated principles, I shall make no apology for offering a few remarks on the subject to the Royal Society.

As soon as the sun has disappeared from any part of the globe, the surface begins to lose heat by radiation, and in greater proportions as the sky is clearer; but the land and water are cooled by this operation in a very different manner: the impression of cooling on the land is limited to the surface, and very slowly transmitted to the interior; whereas in water above 45° FAHRENHEIT, as soon as the upper stratum is cooled, whether by radiation or evaporation, it sinks in the mass of fluid, and its place is supplied by warmer water from below. and till the temperature of the whole mass is reduced

nearly to 40° F. the surface cannot be the coolest part. It follows, therefore, that wherever water exists in considerable masses, and has a temperature nearly equal to that of the land, or only a few degrees below it, and above 45° F. at sunset, its surface during the night, in calm and clear weather, will be warmer than that of the contiguous land; and the air above the land will necessarily be colder than that above the water; and when they both contain their due proportion of aqueous vapour, and the situation of the ground is such as to permit the cold air from the land to mix with the warmer air above the water, mist or fog will be the result; which will be so much the greater in quantity, as the land surrounding or inclosing the water is higher, the water deeper, and the temperature of the water, which will coincide with the quantity or strength of vapour in the air above it, greater.

I shall detail some observations which appear to me to show the correctness of this view. June 9th, 10th, 11th, the temperature of the atmosphere and of the Danube was repeatedly examined during a voyage that I made upon this river from Ratisbonne to Vienna, and on each of these days, the sky being perfectly clear, the appearance of mist above the river in the evening uniformly coincided with the diminution of the temperature of the air from three to six degrees *below* that of the river, and the disappearance of fog in the morning with the elevation of the temperature of the air *above* that of the river. From Ratisbonne to Passau, the temperature of the Danube was pretty uniform throughout the 24 hours, being highest, 62° F. or $62\frac{1}{4}^{\circ}$ F., between 12 and 2 o'clock, and about one degree less before sun-rise, and the temperature of the air from 61° F. to 73° F. during the day,

and from 61° to 54° F. during the night. Below Passau, the Inn and the Ilz flow into the Danube.* On examining the temperature of these rivers at 6 o'clock, A. M. June 11, that of the Danube was found to be 62° F., that of the Inn $56\frac{1}{2}^{\circ}$ F., and that of the Ilz 56° F.: the temperature of the atmosphere on the banks where their streams mixed, was 54° . The whole surface of the Danube was covered with a thick fog; on the Inn there was a slight mist, and on the Ilz barely a haziness, indicating the deposition of a very small quantity of water. About 100 yards below the place where the three rivers joined, the temperature of the central part of the Danube was 59° F., and here the quantity of mist was less than on the bed of the Danube before the junction; but about half a mile below, the warmer water had again found its place at the surface, and the mist was as copious as before the union of the three rivers. June 12th, the evening was cloudy, and the temperature of the atmosphere remained till after dark higher than that of the river, being, when the last observation was made, 63° F. when there was not the slightest appearance of mist. The sky was clearer before sun-rise on the 13th, and the thermometer immediately after sun-rise, in the air above the river, stood at $55\frac{1}{2}^{\circ}$ F. the temperature of the Danube being 61° F.; a thin mist was seen immediately above the river; but there being no mass of vapour to exclude the sun-beams, it rapidly disappeared, and was not visible a few feet from the surface; and in half an hour the whole atmosphere was perfectly transparent.

In passing along the Rhine from Cologne to Coblentz,

* The Danube was greenish, the Inn had a milky blueness, the Ilz was perfectly pellucid; but from the rapidity with which the Inn descended, its waters at this spot give their tint to the whole surface.

May 31st and June 2d and 3d, the nights being very clear, the same phenomenon of the formation of mists was observed, precisely under the same circumstances; but as I could examine the temperature of the air and of the river only close to the banks, and in two or three situations, my observations were less precise and less numerous; the mist formed later in the evening, and disappeared sooner in the morning than on the Danube; which was evidently owing to the circumstances of the atmosphere being warmer and the river colder, the temperature of the one being from 66° F. to 75° F. during the day, and that of the river, where I examined it, from 59° to 60° F.

July 11th. I examined the temperature of the Raab near Kermond in Hungary, at 7 o'clock, P. M. and found it 65° F. that of the atmosphere being 72° F. During the whole evening there were some thin fleecy clouds in the western sky, which being lighted up by the setting sun, greatly interfered with the cooling by radiation from the earth, and at half past nine the thermometer, in the atmosphere, was still 69° F. and at half past ten 67° F., when there was not the slightest appearance of mist. In the morning, before sun-rise, the temperature of the atmosphere on the banks was 61° F., that of the river 64° F., and now the bed of the river was filled with a white thin mist, which entirely disappeared half an hour after sun-rise.

I made similar observations on the Save in Carniola, in the end of August; on the Isonzo in the Friul, in the middle of September; on the Po near Ferrara, in the end of September; and repeatedly on the Tiber and on the small lakes in the Campagna of Rome in the beginning of October; and I have never in any instance observed the formation of mist on a

river or lake, when the temperature of the water has been lower than that of the atmosphere, even when the atmosphere was saturated with vapour.

It might at first view be supposed, that whether the cooling cause existed in the water or the land, the same consequences ought to result ; but the peculiar properties of water, to which I referred in the beginning of the paper, render this impossible. Water in abstracting heat from the atmosphere becomes lighter, and the warmer stratum rests on the surface, and its operation in cooling the atmosphere is extremely slow ; besides, the cooled atmospheric stratum remains in contact with it, and water cannot be deposited from vapour, when that vapour is rising into an atmosphere of a higher temperature than its own ; and the law holds good, however great the difference of temperature. Thus, August 26th, at sun-set, the day after a heavy fall of rain, and when the atmosphere was exceedingly moist, I ascertained the temperature of the Drave near Spital in Carinthia, and though it was 14° F. below that of the air, yet the atmosphere above the river was perfectly transparent.

It may be imagined, that without any reference to the cooling agencies of air from the land, mist may form upon rivers and lakes, merely from the loss of heat by radiation from the air, or the vapour itself immediately above the water ; and that the phenomenon is merely one of the formation of vapour, the source of heat being in the water ; and its deposition, the source of cold, being in the atmosphere ; but it is extremely improbable, that air or invisible vapour, at common temperatures, can lose any considerable quantity of heat by radiation ; and, if mist could be formed from such a

source, it must always be produced to a great extent upon the ocean in calm weather during the night, particularly under the line, and between the tropics, which the journals of voyages sufficiently prove is not the case. I have myself had an opportunity of making some observations which coincide with this view. During a voyage to and from Pola, I passed the nights of September 3, 5, and 6, off the coast of Istria; there was very little wind on either of the nights, and from sun-set till nearly midnight it was perfectly calm in all of them. On the 3d it was cloudy, and the lightning was perceived from a distant thunder storm, and the vessel was never far from the shore: but on the 5th and 6th the sky was perfectly clear, and the zodiacal light, after sun-set, wonderfully distinct and brilliant, particularly on the 5th, and we passed by the help of oars from two to eight miles from the shore. The temperature of the sea at sun-set was 76° F. on the 5th, 77° F. on the 6th, that of the atmosphere immediately above it 78° F. and 79° F. On the 5th, at midnight, about five miles from the shore, the temperature of the sea was 74° F. and that of the atmosphere 75° F., and on the 6th, at the same hour, at about four miles from the shore, the temperature of the sea was 73° F. and that of the atmosphere 75° F. There was not the slightest appearance of mist on either of these nights on the open sea, or at any distance from the land: but close under the hills of Istria there was a slight line of haze visible before sun-rise, which was thickest under the highest land; and as we approached at sun-rise, on the 7th, the mountains of the Friul, the tops of those nearest to Trieste were seen rising out of a thick white mist, which did not reach a quarter of a mile from the shore.

After mists have formed above rivers and lakes, their increase seems not only to depend upon the constant operation of the cause which originally produced them, but likewise upon the radiation of heat from the superficial particles of water composing the mist; which produces a descending current of cold air in the very body of the mist, whilst the warm water continually sends up vapour: it is to these circumstances, that the phenomena must be ascribed of mists from a river or lake, sometimes arising considerably above the surrounding hills. I have often witnessed this appearance during the month of October, after very still and very clear nights, in the Campagna of Rome above the Tiber, and on Monte Albano over the lakes existing in the ancient craters of this extinguished volcano, and, in one instance, on the 17th of October, before sun-rise, there not being a breath of wind, a dense white cloud of a pyramidal form was seen on the site of Alban lake, and rising far above the highest peak of the mountain, its form gradually changed after sun-rise, its apex first disappeared, and its body, as it were, melted away in the sun beams.

Where rivers rise from great sources in the interior of rocks or strata, as they have the mean temperature of the climate, mists can rarely form upon them except in winter, or late in autumn, or early in spring. In passing across the Apennines, October 1st, 2d, and 3d, 1818, there having been much rain for some days preceding, and the nights being very clear, I observed the beds of all the rivers in the valleys filled with mist, morning and evening, except that of the Clitumnus near its source, in which there was no mist, and this river rises at once from a lime-stone bed, and when I examined it,

at half past six o'clock, A. M. October 3, was $7\frac{1}{2}^{\circ}$ lower than the atmosphere.

Great dryness in the air, or a current of dry air passing across a river, will prevent the formation of mist, even when the temperature of the water is much higher than that of the atmosphere: thus on the 14th of June, near Mautern, though the Danube at five in the morning was 61° F. and the air only 54° , yet there was no mist; but a strong easterly wind blew, and from the rapidity with which water evaporated it, it was evident that this wind was in a state of extreme dryness.

The Tiber has furnished me with a number of still more striking examples. October 13th, the night having been very clear, on arriving at the Ponte Molle, at half past six in the morning, I found no mist on the river, yet the temperature of the air immediately above it was 48° F. and that of the river 56° F., a strong north wind blew, which indicated, by the hygrometer, a degree of dryness of 55° , and this part of the river was exposed to it; but the valley above, where the river was sheltered from the wind, was full of mist, and the mist in rising to the exposed level might be seen, as it were, dissolving, presenting thin striæ which never reached above a certain elevation, and many of which disappeared a few seconds after they rose. From the 13th to the 25th of Oct. during which time the tramontane or north wind blew, I witnessed repeatedly the same phenomenon, and in the whole of this time there was only one morning when there was no mist in the sheltered valleys, and the cause was perfectly obvious; the night had been very cloudy, and the thermometer, before sun rise, indicated a difference of only one degree in the atmosphere below that of the river.

It is not my intention to discuss the general subject of the deposition of water from the atmosphere, in this paper ; but merely to describe a local cause of considerable extent and variety in its modifications : and which is not without an effect in the economy of nature, for verdure and fertility, in hot climates, generally follow the courses of rivers, and by the operation of this cause, are extended to the hills, and even to the plains surrounding their banks.

Rome, Dec. 8, 1818.

X. Observations on the Dip and Variation of the Magnetic Needle, and on the Intensity of the Magnetic Force ; made during the late voyage in search of a North West Passage. By Captain Edward Sabine, of the Royal Regiment of Artillery, F. R. S. and F. L. S.

Read February 25, 1819.

THE dipping needle used in these observations is the property of HENRY BROWNE, Esq.; it was made by Messrs. NAIRNE and BLUNT, and is similar in construction to one made by the same artists, and described by the Hon. HENRY CAVENDISH in the 66th volume of the Philosophical Transactions, as used in the house of the Royal Society.

Previously to delivering it into my charge, Mr. BROWNE had adjusted the balance of the needle by means of the screws on the cross of wires attached to its axis ; so that no alteration took place in the indication of the dip, on reversing the poles.

The instrument was placed in the direction of the magnetic meridian, by a compass stationed at a sufficient distance, and suffered to remain during the observations for the purpose of occasional verification. When time admitted, the correctness of adjustment was also proved, by observing the minimum of dip. An equal number of observations were made with the face of the instrument towards the east and towards the west; the arcs indicated at both ends of the needle were read.

In determining the intensity of the magnetic force, the needle was drawn to an horizontal position by a magnet, and being released at an observed moment of time, was suffered

to oscillate until the arcs became too small to be readily distinguished: the first arc was thus equal to the dip, and at every tenth vibration both the arc and time were noted. The observations in the magnetic meridian were repeated with the face of the instrument towards the east and towards the west.

It is highly satisfactory to notice the agreement of the results which were obtained in London and in Shetland, at different periods, and by different observers; showing that the adjustment of the balance of the needle was preserved during the voyage, notwithstanding the accidents to which it was liable: and as a testimony of the excellence of the instrument, and of the confidence which may be placed in observations made with it.

Observations on the dip.

1818.	Latitude.	Longitude.	No. of obser.	Observer.	Dip.	Remarks.
April 13	51° 31' N	0° 08' W.	16	Capt. Kater	70° 34' 39"	Regent's Park, London.
30	60° 09½'	1 12	14	Capt. Sabine	74° 22' 48"	} Brassa Island, Shetland.
May 1	60° 09½'	1 12	12	Lieut. Parry	74° 20' 10"	
June 9	68 22	53 50	12	Capt. Sabine	*83° 08' 07"	On ice.
19	70 26	54 52	14	Capt. Sabine	*82° 48' 47"	Hare Island.
July 8	74 04	57 52	10	Capt. Sabine	84° 09' 15"	(Baffins) three Islands.
23	75 05	60 03	10	Lieut. Parry	84° 24' 57"	
23	75 05	60 03	10	Capt. Sabine	84° 25' 15"	} On ice.
Aug. 2	75 51½'	63 06	10	Capt. Sabine	84° 44' 30"	
4	75 59	64 47	10	Capt. Sabine	84° 52' 06"	On ice.
19	76 32	73 45	10	Capt. Sabine	85° 44' 23"	On ice.
20	76 45	76 00	14	Lieut. Parry	86° 08' 53"	} On ice.
20	76 45	76 00	14	Capt. Sabine	86° 09' 33"	
25	76 08	78 29	16	Capt. Sabine	85° 59' 31"	On ice.
Sept. 11	70 35	66 55	10	Capt. Sabine	84° 39' 21"	On ice.
Nov. 3	60° 09½'	1 12	14	Lieut. Parry	74° 21' 06"	} Brassa Island, Shetland.
3	60° 09½'	1 12		Capt. Sabine	74° 21' 47 15"	
1819. March	51 31	0 08	16	Capt. Sabine	70° 33' 16"	Regent's Park, London.

It is probable that the needle was affected by local attraction either on the 9th or on the 19th of June; but on which day it is difficult to say. On the 9th the ships were anchored to an iceberg of very considerable size, on which the observations were made, the instrument being removed as far as possible from the ships. On the 19th it was used in the observatory which was erected on Hare Island; every fastening of this ingenious and useful building was of brass, and the greatest care was taken to prevent the needle being disturbed by local or accidental causes. But there were several basaltic columns on the face of a hill which rose immediately from the observatory, which may have had an influence; as these columns on Hare Island are said, by Professor GIESECKE,* to have a powerful effect on the needle.

Observations on the intensity of the magnetic force.

Regent's Park, London, April, 1818. By Captain KATER.

<i>Perpendicular to the meridian.</i>				
100	8 21,6	Mean	Account of vibra- tions.	1 0 0 0 91 0 59 0 0 42 0 0 30 0 0 21 0 0 15 0 0 10 0 0 6 0 0 4 0 0 2 0 0 1 0 0
100	8 15		No. of vibra- tions.	0 10 20 30 40 50 60 70 80 90 100
100	8 18,3			

The subsequent observations were made by Captain SABINE.

* Art. Greenland, BREWSTER's Cyclopædia.

Brassa Island, Shetland, lat. $60^{\circ} 09'$, long. $1^{\circ} 12' W$.

<i>In the magnetic meridian.</i>									
Number of vibrations.	Interval.	Mean	Time.	Seconds.					
	m. s		Arc.	74°	53	40	48	5	
100	7 49.5		No. of vibrations.	0	10	20	30	40	50
100	7 50				60	70	80	90	100
100	7 49.75								
<i>Perpendicular to the meridian.</i>									
Number of vibrations.	Interval.	Mean	Time.	Seconds.					
	m. s.		Arc.	90°	57	42	47	48	5
100	8 00		No. of vibrations.	0	10	20	30	40	50
100	7 59				60	70	80	90	100
100	7 59.5								

On an iceberg in Davis's Strait, lat. $68^{\circ} 22'$, long. $53^{\circ} 50' W$.

<i>In the magnetic meridian.</i>									
Number of vibrations.	Interval.	Mean	Time.	Seconds.					
100	m. s.		Arc.	83°	48	45	45	43	44
100	7 17		No. of vibrations.	0	10	20	30	40	50
100	7 23				60	70	80	90	100
100	7 20								
<i>Perpendicular to the meridian.</i>									
Number of vibrations.	Interval.		Time.	Seconds.					
100	m. s.		Arc.	90°	47	46	45	45	44
	7 33		No. of vibrations.	0	10	20	30	40	50
					60	70	80	90	100

On Hare Island, lat. 70° 26', long. 54° 52' W.

<i>In the magnetic meridian.</i>											
Number of vibrations.	Interval.	Mean	Time.	Seconds.							
100	m. s. 7 22		Arc.	83°	48	45	45	44	44	43	43
100	7 24		No. of vibrations.	0	10	20	30	40	50	60	70
100	7 21										
<i>Perpendicular to the meridian.</i>											
Number of vibrations.	Interval.		Time.	Seconds.							
100	m. s. 7 26		Arc.	90°	48	46	45	44	44	43	45
			No. of vibrations.	0	10	20	30	40	50	60	70

On ice in Baffin's Bay, lat. 75° 05', long. 60° 28'.

In the magnetic meridian.																
Number of vibrations.	Interval.	Mean	Time.	Seconds.												
	m s.		Arc.		84½	59	45	45.5	44.5	44.5	44	43	44.5	45	44	43
100	7 29		No. of vibrations.	0	0	10	20	30	40	50	60	70	80	90	100	
100	7 25.5															
100	7 27.5															

Perpendicular to the meridian.																	
Number of vibrations.	Interval.		Time.	Seconds.													
	m. s.		Arc.		90°	61	46	36	28	21	16	12	9	6	4		
100	7 26		No. of vibrations.	0	0	10	20	30	40	50	60	70	80	90	100		

On ice in Baffin's Bay, lat $75^{\circ} 51\frac{1}{2}'$, long. $63^{\circ} 06' W$.

<i>In the magnetic meridian.</i>						
Number of vibrations.	Interval.	Mean	Time.	Seconds.		
	m. s.		Arc.	84°		
100	7 21,5			58	48	
100	7 25			46	45	
				35	44,5	
				40	44,5	
				22	43,5	
				17	43,5	
				13	44	
				10	43,5	
				7	44,5	
				5	44	
100	7 23,25		No. of vibrations.	0	10	20
				30	40	50
				60	70	80
				90	100	
<i>Perpendicular to the meridian.</i>						
Number of vibrations.	Interval.		Time.			
			Arc.			
			No. of vibrations.	0	10	20
				30	40	50
				60	70	80
				90	100	

On ice in Baffin's Bay, lat. $76^{\circ} 45'$, long. $76^{\circ} W$.

<i>In the magnetic meridian.</i>						
Number of vibrations.	Interval.	Mean	Time.	Seconds.		
	m. s.		Arc.	86°		
100	7 13			60	47	
100	7 17			46	45	
				36	44	
				28	43	
				22	43	
				18	43	
				13	42	
				10	44	
				7½	43	
				5	43	
100	7 15		No. of vibrations.	0	10	20
				30	40	50
				60	70	80
				90	100	
<i>Perpendicular to the meridian.</i>						
Number of vibrations.	Interval.		Time.	Seconds.		
	m. s.		Arc.	90°		
100	7 16			45	44	
				35	44	
				27	44	
				21	43	
				17	43	
				13	43	
				9½	44	
				7	43	
				5	44	
				3½	44	
100			No. of vibrations.	0	10	20
				30	40	50
				60	70	80
				90	100	

On ice in Baffin's Bay, lat. 76° 08½', long. 78° 21' W.

<i>In the magnetic meridian.</i>						
Number of vibrations.	Interval.	Mean	Time.	Seconds.		
	m. s.		Arc.	85°		
100	7 15		No. of vibrations.	0	10	45
100	7 17			20	30	44,5
100	7 16			30	40	43,5
				40	50	44
				50	60	43
				60	70	43
				70	80	43
				80	90	42,5
				90	100	44,5
<i>Perpendicular to the meridian.</i>						
Number of vibrations.	Interval.	Mean	Time.	Seconds.		
	m. s.		Arc.	90°		
100	7 18		No. of vibrations.	0	10	46,5
				20	30	45
				30	40	44,5
				40	50	44,5
				50	60	43,5
				60	70	43,5
				70	80	43,5
				80	90	43
				90	100	43,5
				100		42,5

On an iceberg in Davis's Straits, lat. 70° 35', long. 66° 55' W.

<i>In the magnetic meridian.</i>						
Number of vibrations.	Interval.	Mean	Time.	Seconds.		
	m. s.		Arc.	85°		
100	7 16		No. of vibrations.	0	10	46
100	7 16			20	30	44
100	7 16			30	40	44
				40	50	44
				50	60	43
				60	70	43
				70	80	43,5
				80	90	43,5
				90	100	43
				100		42
<i>Perpendicular to the meridian.</i>						
Number of vibrations.	Interval.	Mean	Time.	Seconds.		
	m. s.		Arc.	90°		
100	7 18,5		No. of vibrations.	0	10	46
				20	30	45,5
				30	40	44
				40	50	44,5
				50	60	43,5
				60	70	43,5
				70	80	43,5
				80	90	43
				90	100	41,5
				100		41,5

1819, March. In the Regent's Park, London, lat. $51^{\circ} 31' 40''$,
long. $0^{\circ} 08' W$.

In the magnetic meridian.					
Number of vibrations.	Interval.	Mean	Time.	Seconds.	
	m. s.		Arc	71°	
100	8 01,5		No. of vibrations.	0	50
100	8 08			10	49
100	7 56,5			20	48
100	8 02			30	48
				40	48
				50	47
				60	48
				70	48,5
				80	47
				90	47
				100	47
Perpendicular to the meridian.					
Number of vibrations.	Interval.		Time.	Seconds.	
	m. s.		Arc.	90°	
100	8 18,5		No. of vibrations.	0	52
				10	52
				20	49,5
				30	49,5
				40	49
				50	48,5
				60	49,5
				70	48,5
				80	50,5
				90	49,5
				100	49,5

Abstract of the times in which 100 vibrations were performed.

Latitude. N.	Longi- tude, W.	In the meridian.	First Arc.	Perpendicular to the meridian.	First Arc.	
°	'	m. s.		m. s.	°	
51 31	0 08	0 0	0	8 18,3	90	Regent's Park, London.
60 09	1 12	7 49 $\frac{3}{4}$	74	7 59,5	90	Shetland.
68 22	53 50	7 20	83	7 33	90	On ice, Davis's Straits.
70 26	54 52	7 21	83	7 26	90	Hare Island.
75 05	60 23	7 27 $\frac{1}{2}$	84	7 26	90	On ice, Baffin's Bay.
75 51 $\frac{1}{2}$	63 06	7 23 $\frac{1}{2}$	84	0 0	—	On ice, Baffin's Bay.
76 45	76 00	7 15	85	7 26	90	On ice, Baffin's Bay.
76 08	78 21	7 16	85	7 18	90	On ice, Baffin's Bay.
70 35	66 55	7 16	83	7 18,5	90	On ice, Davis's Straits.
51 31	0 08	8 02	70	8 18	90	Regent's Park, London.

The 100th vibration never exceeded an arc of 3°

Observations to determine the variation of the needle, in Davis's Strait, and Baffin's Bay.

The azimuth compasses used in these observations were constructed on an improved plan, the invention of Captain KATER. It is thus described in the "Instructions for the use of the Instruments furnished to the Northern Expeditions," printed by order of the Royal Society :

"The compass is five inches diameter ; by means of an inclined mirror and lenses, the degrees are seen by reflection considerably magnified ; a line drawn on a piece of ivory is viewed at the same time, and serves as an index by which the degrees are to be read off.

"At the opposite side of the box is a sight on which slides, in a frame, the segment of a glass cylinder, ground to a radius of five inches. By means of this, a fine line of light is thrown on the index, and may be seen at the same time as the degrees on the card.

"The degrees on the card are read from the north towards the east, and are carried round to 360° , in order to obviate the possibility of error in this respect."

The observations were made either on shore or on the ice, sufficiently distant from the ship to be beyond the influence of her attraction. The compass was placed on a copper fastened stool, and was carefully levelled by means of a spirit level, to ensure the perpendicularity of the sight vane.

Each altitude and azimuth is a mean of several observations, the compass being removed and levelled afresh between every one, thus making each faithfully distinct.

The mean Greenwich time is given, as it determines the amount of the sun's polar distance.

The latitudes and longitudes are of the spot, deduced by the ship's log from the nearest observed.

The altitudes are corrected for index error only, the letters or signs annexed denote the limb, and whether by reflection or by the natural horizon.

The observed azimuths are of the sun's centre cleared of index error; the compasses used were No. 1 and 2, supplied to the *Isabella*, and No. 3 to the *Alexander*; the true azimuths deduced from the elements contained in the preceding columns, are expressed in a corresponding manner to the reading of the compass, for the purpose of comparison.

The observations were made on ice, except when otherwise noted in the column of remarks.

When due consideration is given to the greatly diminished power, with which the earth's magnetism acts on the horizontal direction of the needle, when the dip becomes so considerable as it was found in Davis's Straits and Baffin's Bay, namely, from 83° to 86° ; the satisfactory results which have been obtained, even under such extreme circumstances with Captain KATER's compasses, afford the best testimony of their excellence, and of the precision which may be expected from them in the ordinary course of observation.

It may also be remarked, that a difference in the result of azimuths observed at different hours of the day may not be altogether an error of observation, since it is probable that as the directive power of magnetism diminished, the causes which produce the hourly change in the variation itself may act with increased effect.

Should the amount of this change be considerably augmented in high magnetic latitudes, careful observations on the direction of the needle at different hours of the day, on all convenient occasions, might be serviceable towards a more certain knowledge of its causes, than has been hitherto obtained from observations made where the effects are so inconsiderable.

The influence of the ship's iron on their compasses increasing, as the directive power of magnetism diminished, produced irregularities that rendered observations on board ship of little or no value towards a knowledge of the true variation; a few azimuths which were observed in the *Isabella*, have been selected for the purpose of exemplifying this remark. They will also show, how essential it is to navigation in high latitudes, that the nature of the errors which the ship's attraction produces in her compasses, should be understood.

Observations to determine the variation of the needle, made on shore, or on the ice. Observer,
Captain SABINE.

1818.	Mean Green- wich time.	Latitude.	Longitude.	Observed Altitude.	Observed azimuth.	Compass.	True azimuth.	VARIATION.	Remarks.
	h. m. s.	° ' " N.	° ' " W.	° ' " Q.	° ' "		° ' "	° ' "	
June 9	22 0 0	68 23 1/2	53 47 W.	48 23 15 Q	156 27	1	88 55 1/2	67 31 1/2 W.	
11	23 20 0	68 14	54 15	63 20 58 Q	175 47	2	108 07	67 40	
12	9 0 0	68 14	54 15	70 02 33 Q	186 16	1	118 12	68 04	
17	8 20 0	70 26 1/2	54 52	56 15 36 Q	332 54	1	260 08	72 46	
17	8 30 0	70 26 1/2	54 52	55 32 34 Q	334 05	1	261 11 1/2	72 53 1/2	
18	8 28 0	70 26 1/2	54 52	55 20 52 Q	332 52	1	261 33	71 19	
18	8 35 0	70 26 1/2	54 52	{ 55 33 45 Q } { 53 57 35 Q }	334 24	1	263 10 1/2	71 13 1/2	Observatory, Hare Island.
18	23 20 0	70 26 1/2	54 52	64 43 18 Q	184 35	1	112 55 1/2	71 39 1/2	
27	9 00 00	71 02 1/2	54 13	50 18 13 Q	343 26 1/2	1	268 13 1/2	75 13	
27	9 20 00	71 02 1/2	54 13	49 18 20 Q	346 33 1/2	1	271 13	75 20 1/2	
27	9 40 00	71 02 1/2	54 13	47 07 0 Q	349 54 1/2	1	274 23	75 31	
27	10 00 00	71 02 1/2	54 13	44 36 40 Q	353 35	1	278 0	75 35	
27	10 20 00	71 02 1/2	54 13	42 07 17 Q	357 24 1/2	1	281 36 1/2	75 48	
July 4	10 45 00	72 44 1/2	56 49	37 18 47 Q	7 13 1/2	1	288 18 1/2	78 55	
6	9 12 0	73 22 1/2	57 32	49 10 00 Q	348 16	1	268 15	80 01	
12	23 30 0	74 01 1/2	57 52	55 11 52 Q	186 49 1/2	1	106 05 1/2	80 43 1/2	On Baffin's three Islands.
21	10 05 0	74 58	59 16	38 49 57 Q	0 33 1/2	1	276 00 1/2	84 33	
22	23 02 0	75 04	60 03	45 46 0 Q	185 16	1	98 16 1/2	86 59 1/2	
28	10 20 0	75 28	60 34 1/2	35 04 10 Q	7 40	1	279 21 1/2	88 18 1/2	
30	8 17 0	75 32	61 0	48 23 57 Q	336 24 1/2	1	248 46 1/2	87 37 1/2	
30	10 20 0	75 32	61 0	36 29 35 Q	0 55 1/2	1	272 41 1/2	88 13 1/2	
Aug. 2	10 00 00	75 44 1/2	64 0	17 33 15 L.	2 26	1	273 25	89 01	
2	10 10 00	75 44 1/2	64 0	17 01 25 L.	4 31	1	275 37 1/2	88 53 1/2	
4	0 12 30	75 59	64 32	46 39 55 Q	204 01	1	113 43 1/2	90 17 1/2	
6	9 34 0	70 50 1/2	64 34	37 27 20 Q	354 24	1	263 16 1/2	91 07 1/2	
12	0 00 51	75 54 1/2	65 30	40 19 34 Q	202 31 1/2	1	109 45 1/2	92 45 1/2	Compasses used alter- nately.
12	0 12 51	75 54 1/2	65 30	40 59 55 Q	205 24 1/2	2	110 50 1/2	94 34 1/2	
12	0 29 30	75 54 1/2	65 30	43 36 28 Q	210 52 1/2	1 & 2	117 12 1/2	93 39 1/2	
19	2 13 00	76 30	72 35	45 59 50 Q	240 32	1 & 2	138 23	102 09	
19	2 33 00	76 30	72 35	47 27 10 Q	246 53 1/2	2	143 37 1/2	103 16	
19	2 33 00	76 30	72 35	47 33 40 Q	245 58 1/2	1	144 03 1/2	101 55	
22	9 00 00	76 32 1/2	76 52 1/2	36 54 40 Q	348 56	1	241 00	107 56	
25	8 27 10	76 08 1/2	78 21	40 32 30 Q	341 05 1/2	2	230 07	110 58 1/2	
25	8 27 10	76 08 1/2	78 21	40 14 05 Q	341 17 1/2	1	230 58	110 19 1/2	
25	10 03 00	76 08 1/2	78 21	30 06 47 Q	4 05	1	254 56	109 09	Observed by the silk line, the line of light not being per- ceptible owing to the weather.
25	10 08 00	76 08 1/2	78 21	28 25 25 Q	5 43 1/2	3	256 18	109 25 1/2	
Sept. 11	8 30 00	70 35 1/2	66 55 1/2	13 27 30 L.	330 44	1 & 2	244 06	86 38	
17	8 36 00	70 35 1/2	66 55 1/2	12 59 47 L.	332 58 1/2	2	245 36	87 28 1/2	Dip of horizon allowed for 5 1 feet 7 in. measured.
11	8 40 00	70 35 1/2	66 55 1/2	12 39 33 L.	333 19 1/2	1	246 41	86 38 1/2	

In the column of "observed altitude," Q signifies the lower, and Q the upper limb of the sun, the altitude being taken by reflection; L, the lower limb by the natural horizon.

Results of azimuths observed on board the Isabella, with WALKER's azimuth compass, placed amidships in front of the companion.

1818.	Latitude.	Longitude.	Ship's head.	Variation.	Remarks.
June 3	65° 38'	54° 24'	NW. by W.	67° 10'	True variation observed on the ice 75° 30'.
4	65 47	54 44	N.	66 22	
4	65 47	54 44	W.	77 34	
4	65 46	54 44	E. S. E.	47 54	
5	65 47	54 22	N.W.	76 47	
5	65 47	54 22	N.	67 32	
7	65 50	55 0	NE. by E.	49 57	
27	74 02	54 13	E.	64 56	
27	74 02	54 13	S. E.	67 07	
27	74 02	54 13	S.	76 27	
27	74 02	54 13	S.W.	84 38	
27	74 02	54 13	W.	93 16	
27	74 02	54 13	N.W.	90 20	
27	74 02	54 13	N.	77 44	
27	74 02	54 13	N.E.	70 30	
Aug. 29	74 44	77 50	W.N.W.	128 35	True variation observed on the ice 86° 53'.
30	74 21	78 0	N. by E.	104 28	
Sep. 3	73 55	73 47	N. by W. $\frac{1}{2}$ W.	108 55	
4	73 23	75 58	E. by S. $\frac{1}{2}$ S.	85 04	
5	72 34	74 06	S.E.	74 22	
5	72 34	74 06	S. $\frac{1}{2}$ W.	90 32	
5	72 34	74 06	S. S. E.	78 50	
11	70 37	66 58	N.W.	98 42	
11	70 37	66 58	N.W. by W.	100 08	
11	70 37	66 58	W.N.W.	100 56	
11	70 37	66 58	W. by N.	99 36	
16	68 40	64 0	N. by E.	73 56	
16	68 40	64 0	S.E. by E. $\frac{1}{2}$ E.	59 07	
29	64 53	62 10	N.N.W.	71 21	
29	65 02	62 10	S.W.	75 40	
Oct. 14	59 30	36 07	E. S. E.	39 04	
14	59 30	36 07	N.W. by N.	52 55	

XI. *On the action of crystallized surfaces upon light.* By David Brewster, LL. D. F. R. S. Lond. and Edin. In a letter addressed to the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S. &c. &c. &c.

Read February 25, 1819.

MY DEAR SIR,

IT has been remarked by MALUS, in his Theory of Double Refraction, “that the action which the first surface of Iceland spar exercises upon light, is independent of the position of its principal section ;—that its reflecting power extends beyond the limits of the polarising forces of the crystal, and that as light is only polarised by penetrating the surface, the forces which produce extraordinary refraction begin to act only at this limit.” He also observes, that “the angle of incidence at which Iceland spar polarises light by partial reflection, is $56^{\circ} 30'$; that it then comports itself like a common transparent body ; and that whatever be the angle comprehended between the plane of incidence and the principal section of the crystal, the ray reflected by the first surface is always polarised in the same manner.” *

These conclusions, obtained experimentally by an author of such distinguished eminence, I should naturally have received as established truths, had I not been led, by a series of experiments made before the perusal of his work, to opinions of an opposite kind. My experiments indicated an extension of the polarising forces *beyond* the crystal ; and I

* *Théorie de la Double Refraction*, pp. 240, 241.

was thus induced to question the accuracy of MALUS's views, and to repeat the experiments upon which he had founded them. The results of this investigation, while they have overturned the opinions hitherto adopted, have at the same time led to the establishment of several points both of theoretical and practical importance.

In giving an account of these results, I shall *first* consider the effects produced upon transmitted light by a change in the mechanical condition of the surfaces of crystals, and then establish the laws according to which the interior forces affect and modify the forces which produce partial reflection.

*

SECT. I. *On the effects produced upon transmitted light, by a change in the mechanical condition of the surfaces of crystals.*

If we take a hexaedral prism of *nitrate of potash*, and observe a luminous object through two of its inclined surfaces that have a good natural or artificial polish, we shall perceive two distinct and perfectly formed images. If we now roughen these two surfaces, and cement upon each of them a plate of glass by means of *balsam of capivi*, the character of the two images will be greatly changed. The image that has suffered the greatest refraction will be as distinct as before, but the other image will be either of a faint reddish colour, or wholly invisible, according to the degree of roughness induced upon the refracting surfaces. When *oil of cassia* is used instead of the balsam, the least refracted image, if it was visible before, will now be completely extinguished.

By substituting pure *alcohol*, or the *white of an egg*, instead of the balsam, the least refracted image will become distinct, and the most refracted image will be either a mass of nebulous,

light, or almost invisible. A result nearly similar will be obtained with water, notwithstanding its effect in dissolving the little prominences which constitute the superficial roughness of the crystal.

In order to explain these phenomena, we must recollect that the index of refraction for the ordinary image of nitre is 1.511, and that of the extraordinary image 1.328. When the rough surface of the nitre is covered with balsam of capivi, which has nearly the same index of refraction as the ordinary image, the same effect is produced as if the rough surface had been polished for the ordinary rays. All the little pits or depressions in the rough surface being filled up with balsam, the ordinary rays suffer little or no refraction in penetrating the crystal, and therefore the image which they form will be as clear and distinct as in the first experiment. But since the index of refraction for the extraordinary image is much less than that of the balsam, the rays of which it is composed will not enter the crystal undisturbed, but will be scattered in the same manner as if its surface was rough, and had a refractive power corresponding to the difference between the index of refraction for the extraordinary ray, and the index of refraction for the balsam. When water or alcohol is substituted in room of the balsam, the effects now described are interchanged, the roughness being removed for the extraordinary rays by the application of a fluid of the same refractive density, while the rays that form the ordinary image are dispersed by the refractions which still exist at the rough surface of the crystal.

These effects will be still better understood by supposing the crystal to consist of an extraordinary and an ordinary

medium, arranged in alternate strata. When the superficial polish of both these media is removed, the application of the *balsam* restores, as it were, the polish of the ordinary medium, without restoring that of the extraordinary medium; while the application of the *alcohol* restores the polish of the extraordinary medium without restoring that of the ordinary medium.

When the refractive power of the fluid is intermediate between that of the two media, the ordinary and the extraordinary image will be equally indistinct; and we have it in our power to alter the distinctness of either of the images, by varying the refractive force of the interposed fluid.

If the plane of incidence is not perpendicular to the axis, a variation in the angle of incidence will produce a variation in the index of refraction of the extraordinary ray; and, since the refractive power of the interposed fluid suffers no change, the extraordinary image must become more or less distinct, according as its index of refraction is made to approach more or less to that of the fluid, by varying the inclination of the refracted ray to the axis.

From the preceding experiments, which have been repeated with the same results with *calcareous spar*, *arragonite*, and many other crystals, we may deduce the following conclusions:

1. The force of double refraction and polarisation extends not only without the interior limit of the ordinary refractive force, but also without the surface of the crystal.
2. The force of double refraction and polarisation emanates from the surface of bodies, though its intensity depends upon the inclination of the surface to the axis of the crystal.
3. The ordinary or the extraordinary image may be ex-

tinguished at pleasure in any doubly refracting crystal; and the crystal is thus converted into a singly refracting crystal, like certain specimens of agate.

4. In soft crystals that do not admit of a perfect polish, the distinctness of any of the two images may be made a maximum, by giving the crystal the best polish of which it is susceptible, and then cementing plates of glass upon its surfaces, by a transparent cement of the same refractive power as that of the pencil which is to be rendered most distinct. If it is required to make the two images equally distinct, the refractive power of the cement must be a mean between that of the ordinary refraction, and the extraordinary refraction which corresponds to the angle which the refracted ray forms with the axis of double refraction.

5. All doubly refracting crystals consist of an ordinary and an extraordinary medium, alternating with each other, and varying in density according to a law which I have described in another paper.*

I consider the optical structure of *agate* as demonstrating the existence of two media. In quartz, the two media are equally perfect and transparent; but in certain specimens of agate, the one medium is seen in a separate state from the other, and broken down into small portions like the figures 333. The light which passes through these portions, is evidently acted upon by a different refractive power from that which

* The paper here alluded to, was laid before the Royal Society of Edinburgh on the 16th of March, 1818; but as it could not have been understood without the preceding experiments, its publication was necessarily delayed. The theory which it contains embraces also the complex phenomena which arise from the combination of two or more axes. See the *Phil. Trans. Lond.* 1818, p. 264.

passes through the rest of the crystal, and is polarised in a transverse plain.*

SECT. II. *On the influence of the polarising force of doubly refracting crystals, upon the polarising force which accompanies partial reflection.*

The experiments in the preceding section could not fail to throw a doubt upon the identity of action exercised upon reflected light by crystallized and uncrystallized surfaces. I was therefore led to a more minute investigation of the subject, and obtained a series of very unexpected results, which I shall explain under the three heads into which they naturally arrange themselves.

1. *On the change produced upon the polarising angle by the interior forces of doubly refracting crystals.*

In order to examine with care the superficial action of calcareous spar, I exposed several surfaces by cleavage, and having selected the one that had the most perfect polish, I covered all the other sides of the rhomb with black wax, and measured the polarising angles in planes variously inclined to the principal section. The following are the results of a great number of observations :

Position of the crystal.	Azimuth.	Polarising angle.	No. of observations.
Short diagonal in plane of reflection	0°	57° 14'	39
One of the edges in plane of reflection	50	57½' 58 32	15
Long diagonal in plane of reflection	90	59 32	37
Difference between the greatest and least polarising angle			
-	-	-	2° 18'

* See *Phil. Trans.* 1813, p. 104; 1814, p. 191; and *Edin. Trans.* vol. vii. p. 298, 9.

The following observations were made with rhombs taken from a different mass of calcareous spar :

Position of the crystal.	Azimuth.	Polarising angle.	No. of observations.
Short diagonal in plane of reflection	0°	57° 36'	5
One of the edges in plane of reflection	50 57½'	58 50	7
Long diagonal in plane of reflection	90	59 44	7

In these experiments the results were the same, whether the obtuse angle of the rhomb was nearest or farthest from the eye, or whether it was to the right or left hand of the observer.

In order to determine the angle of polarisation for surfaces differently inclined to the axis, I selected some fine Farøë crystals of calcareous spar from the cabinets of Sir GEORGE MACKENZIE and Mr. ALLAN. One of these crystals was an acute dodecahedron, having a highly polished surface inclined about 5° to the axis, and with it I obtained the following results.

Position of the crystal.	Polarising angle.	No. of observations.
Axis in plane of reflection -	54° 18'	13
Axis perpendicular to plane of reflection - - -	58 14	10

I was now desirous to obtain a surface perpendicular to the axis; but I have searched in vain for such a specimen, and have invariably found that the summit of the prism is rough and unpolished. With a surface polished by art, and cut perpendicular to the axis, I found the polarising angle about 58° 15'; but I do not regard this result as deserving any particular attention.

In order to supply the defect of natural faces, I ground and polished a great variety of surfaces, inclined at all angles,

to the axis; but the results clearly proved that the peculiar action of the surfaces, in varying the polarising angle, is exhibited only by the highly polished faces which are sometimes obtained from cleavage, or which occur in perfect crystals.

The following observations were made with a fine crystal of *Chromate of lead*:

Position of the crystal.	Polarising angle.	No. of observations.
Axis of prism in plane of reflection	$67^{\circ} 48'$	4
Axis of prism perpendicular to the plane of reflection	- $65^{\circ} 42'$	4

In the first of these positions a great quantity of brilliant blue light remained unpolarised, whereas in the second position the whole of the pencil suffered complete polarisation.

2. *On the change produced upon the polarisation of the reflected ray, by the interior forces of doubly refracting crystals.*

Since the extraordinary force in calcareous spar was thus shown to extend to such a distance beyond the surface as to modify the polarising angle produced by superficial reflection, it became extremely probable that the polarisation of the reflected ray might suffer some change from the same cause: but after the most careful observation, I could not discover the slightest indication of such an effect. Upon reflecting farther, however, on the nature of the change which I had expected, it occurred to me that the action of the ordinary reflecting force was so powerful, as to mask the influence of the inferior force which emanated from the axis, and that the effect of the one might be rendered visible by diminishing the intensity of the other. I accordingly introduced a film of oil of Cassia between a glass prism and the surface of the spar,

and having inclined the prism at a very small angle to that surface, I thus separated the image formed at the common surface of the prism, and the oil from the image formed at the common surface of the oil and the spar. The effect was exactly what I had anticipated. The influence of the ordinary reflecting force was reduced almost to nothing, and the light reflected from the separating surface of the oil and the spar, was polarised at an angle of about $45\frac{1}{2}^{\circ}$, and was almost entirely under the dominion of the force which emanated from the axis. The following were the results obtained with an ordinary surface, inclined $45^{\circ} 29\frac{1}{2}'$ to the axis.

1. *Azimuth* 0° . When the plane of the principal section is in the plane of reflection, the light reflected at the surface of the oil and the spar is polarised in the plane of reflection, the obtuse solid angle being farthest from the eye. The light of the image is of a faint red colour, and has very little intensity.

2. *Azimuth* 12° . The obtuse angle being farthest from the eye, the reflected pencil is polarised about 45° out of the plane of reflection.

3. *Azimuth* 42° . The reflected pencil is polarised transverse to the plane of reflection, or 90° out of it. The light is now of a yellowish white tint, and is much more intense than in azimuth 0° .

4. *Azimuth* 90° . When the plane of reflection is perpendicular to the plane of the principal section, the obtuse solid angle being either to the right or left hand, the reflected pencil is polarised a little more than 135° , or -45° out of the plane of reflection. The intensity of the pencil is now intermediate between that of azimuth 0° and 45° .

5. *Azimuth* 180° . The obtuse angle being now next the

eye, the pencil is polarised 180° out of the plane of reflection, or it has again returned into that plane.

In passing through the last 45° of azimuth, the polarisation varies very slowly, the change being only about 10° ; whereas in passing through the first 42° of azimuth, the polarisation varies no less than 90° , indicating in the most unequivocal manner, as we shall afterwards see, that this change depends upon the angle which the incident ray forms with the axis of the crystal.

The light reflected from the separating surface of the oil and the spar is a *maximum*, when the plane of the principal section is perpendicular to the plane of reflection, and its colour is then nearly white. When these two planes coincide, the intensity of the light is a *minimum*, and its colour is then a faint red; and in intermediate positions, the reflected pencil has both its intensity and its colour of an intermediate character. In the azimuth of 42° , the reflected pencil exhibits a very curious phenomenon when analysed with calcareous spar. Its colour is then yellowish white, and all the *yellow* light is polarised transversely to the plane of reflection. One of the images, however, instead of vanishing, consists of *blue* and *red* light, the *red* vanishing, and the *blue* becoming more brilliant as the analysing prism is turned to the *left*; and the *blue* vanishing, and the *red* becoming more brilliant as the prism is turned to the *right*. This effect arises from the difference in the angles at which the red and blue rays are incident upon the separating surface of the oil and the spar. Each set of rays, therefore, as will afterwards appear, suffers a different change of polarisation, the one being polarised about 87° out of the plane of reflection, and the other 99° .

I have repeated the preceding experiments by substituting in place of oil of cassia, *water*, *alcohol*, *castor oil*, *balsam of capivi*, and *oil of anise seeds*, a series of fluids whose refractive powers increase progressively. With *water*, the light refuses to be polarised completely in the direction of the long diagonal, while it suffers complete polarisation in the direction of the short diagonal. With *alcohol*, the direction of the polarisation is not altered. With *castor oil*, the intensity of the light is greater in the direction of the long diagonal, than in that of the short one; and in the former case, the pencil is polarised at a much greater angle than in the latter. With *balsam of capivi*, in the azimuth of 45° , the pencil is polarised about 15° out of the plane of reflection. In the azimuth of 90° , the pencil is not completely polarised at any angle, but is nearly so in the plane of reflection, and at a considerable angle of incidence. In 0° of azimuth, the pencil is completely polarised in the plane of reflection. With *oil of anise seeds*, in azimuth 45° , the pencil is polarised about 45° out of the plane of reflection. In azimuth 90° , the pencil refuses to be polarised at any angle, and in 0° of azimuth, the polarisation is complete in the plane of reflection.

As the preceding results were obtained with a surface inclined $45^\circ 23\frac{1}{2}'$ to the axis, I was anxious to observe the effects produced by the Faroë crystals, where the natural faces are nearly in the plane of the axis. I accordingly repeated the experiments with a variety of these crystals, and in every case I observed the same phenomena. In the azimuth of 90° , where the polarising angle is $58^\circ 14'$, the pencil was polarised a degree or two out of the plane of reflection. In the azimuth of 45° , where the polarising angle is about

$56^{\circ} 16'$, the change of polarisation is about 40° ; and in the azimuth of 0° , where the polarising angle is $54^{\circ} 18'$, the change of polarisation was a little more than 90° when the obtuse angle was farthest from the eye, and about 106° when the obtuse angle was nearest the eye. In all these positions the image reflected from the surface of the oil and the spar, is nearly as bright as that from the surface of the prism and the oil.

In order to determine the change of polarisation when the plane of reflection was perpendicular to the axis, it was necessary to have a prismatic crystal of calcareous spar with a polished summit; but I have always found this summit rough and unpolished. There was therefore no alternative but to polish an artificial face cut in this direction; and upon the application of oil of cassia, I found that in every azimuth the change of polarisation was about 75° . The colour of the image was a bright yellow, and a little blue light remained at the point of evanescence.

In extending these experiments to other crystals I have obtained similar results; but there are none so well fitted for this species of examination as calcareous spar. In applying oil of cassia to a very fine prism of chromate of lead, the direction of the polarisation was not in the slightest degree altered, as the ordinary action was not sufficiently weakened to render visible the influence of the interior force. When the plane of reflection passed through the axis of the prism, *blue* light remained in the vanishing image; but in a plane rectangular to this, the light was completely polarised, as in the experiment when the reflecting surface was in contact with air.

I now tried *rock crystal* and *oil of anise seeds*, which have nearly the same mean refraction ; but on account of the great debility of the interior polarising force, it was not able to overpower or even to modify that which accompanies partial reflection. I could easily have reduced this last force still farther till it came under the dominion of the first ; but the reflecting power would have been reduced in the same proportion, and would not have been capable of driving back a number of rays sufficient to form a perceptible image. But though the polarisation is not changed at the separating surface of the oil and the rock crystal, yet the character of the reflected light is modified in a very remarkable manner. When the plane of reflection from one of the sides of the prism of rock crystal was in the direction of the axis, or in 0° of azimuth, the reflected image was a *deep blue* of very little intensity ; whereas in a rectangular direction, where the azimuth was 90° , it was of a brick red colour, and much more luminous. On one of the faces of the pyramid, in azimuth 0° , the tint was a *brilliant pink*, intermediate between the *red* and the *blue* ; and on the same face, in 90° of azimuth, it was of a *brick red* colour as before. These variations are obviously related to the axis of double refraction, and indicate the extension of its force within the sphere of partial reflection. The origin of the colours themselves, I shall soon have occasion to explain, in a paper on the action of uncrystallized surfaces.*

3. *General results deduced from the preceding experiments.*

Had it been in my power to command a series of the most

* This Paper was read before the Royal Society of Edinburgh, on the 4th January 1819.

perfect crystals, or to communicate to artificial faces that high polish which nature often exhibits, I might have obtained a more complete generalisation of the preceding phenomena. Limited, however, as the investigation has been by these causes, it still presents us with several views of great generality and interest.

FIRST. The force of double refraction and polarisation extends without the surface of crystals, and within the sphere of the force which produces partial reflection.

SECOND. The change in the angle of polarisation produced by the interior force, depends on the inclination of the reflecting surface to the axis of the crystal, and also on the azimuthal angle which the plane of reflection forms with the principal section.

In any given surface, where A and A'' are the minimum and maximum polarising angles, viz. in the azimuth of 0° and 90° , the polarising angle A' at any intermediate azimuth α , may be found by the formula

$$A' = A + \sin.^2 \alpha (A'' - A).$$

In the rhomboidal surfaces of calcareous spar

$$A'' - A = 138'.$$

THIRD. The change in the direction of the polarisation must be produced after the ray has suffered reflection; for if the change preceded reflection, the reflecting force would have polarised it in the plane of reflection, whatever had been the direction of its previous polarisation.

FOURTH. The change in the direction of the polarisation depends upon the angle which the incident ray forms with the axis of the crystal, and takes place in such a manner that if

ϕ = angle of incident ray with the axis ; and

C = change in the direction of the polarisation,
we shall have

$$\text{Sin. } \frac{1}{2} C = \sqrt{\text{Sin. } \phi}.$$

If we make

A = complement of the inclination of the reflecting plane
to the axis ;

α = azimuth of the plane of incidence with the principal
section ; and

i = angle of incidence reckoned from the perpendicular,
we shall have

$$\text{Cos. } \alpha \times \text{Tang. } A = \text{Tang. } z, \text{ and}$$

$$\text{Cos. } \phi = \frac{\text{Cos. } A \times \text{Cos. } (i \pm z)}{\text{Cos. } z}.$$

In one of the ordinary rhomboidal surfaces where the inclination to the axis is $45^\circ 23\frac{1}{2}'$, $A = 44^\circ 36\frac{1}{2}'$; and with oil of cassia i or the incidence of the mean ray, when the polarisation is complete, is about $45^\circ 17'$. I have assumed it at $45^\circ 23\frac{1}{2}'$ (which will be more correct for the mean luminous ray than $45^\circ 17'$) for the purpose of making the change of polarisation commence with zero in 0° of azimuth.

Upon these principles I have computed the following table, which shows the change in the direction of the polarisation, corresponding to any azimuth and any inclination of the incident ray with the axis.

TABLE showing the change in the direction of the polarisation in different azimuths.

Azimuth.	Inclination of incident ray to the axis.	Change in the direction of the polarisation.
°	°	°
0	0 0	0 0
10	6 54	40 36
20	16 50	65 6
30	23 0	77 22
40	29 24	88 52
45	32 38	94 34
50	36 29	100 54
60	42 17	110 10
70	48 32	120 0
80	54 37	129 8
90	60 0	137 0
100	65 56	145 48
110	71 4	153 4
120	75 42	159 48
129	79 28	165 3
135	81 41	169 46
140	83 22	170 34
150	86 12	174 52
160	88 18	177 36
170	89 33	179 28
180	90 0	180 0

The results in the preceding table enable us to explain the phenomenon described in p. 154. As the interposed oil of cassia has a prismatic form and a very high dispersive power, the blue and the red rays are incident at different angles with the axis, and therefore the change in the direction of their polarisation must be different. The nearest approximation to evanescence in one of the images, belongs to the mean ray of the spectrum, and therefore at this point the image that should have vanished, must consist of blue and red light, one of which will disappear *before*, and the other *after*, the mean ray.

I have the honour to be, &c. &c. &c.

DAVID BREWSTER.

To the Right Hon. Sir JOSEPH BANKS, Bart.
G. C. B. P. R. S. &c. &c. &c.

Edinburgh, Nov. 12, 1818.

From the Press of
W. BULMER & Co.
Cleveland-row, St. James's,
London.

5. Corrections for Refraction.

1. A simple and convenient method of calculating the precise magnitude of the atmospherical refraction, in the neighbourhood of the horizon, has generally been considered as almost unattainable; and Dr. BRINKLEY has even been disposed to assert the "impossibility of investigating an exact formula," notwithstanding the "striking specimens of mathematical skill, which," as he justly observes, "have been exhibited in the inquiry." We shall find, however, that the principal difficulties may be evaded, if not overcome, by some very easy expedients.

2. The distance from the centre of the earth being represented by x , and the weight of the superincumbent column by y , the actual density may be called z , and the element of y will vary as the element of x and as the density conjointly; consequently $dy = -mzx dx$; the constant quantity m being the reciprocal of the modulus of elasticity. The refractive density may be called $1 + px$, p being a very small fraction; and it is easy to see that the perpendicular u , falling on the direction of the light, will always vary inversely as the refractive density, since that perpendicular continually represents the sines of the consecutive angles, belonging to each of the concentric surfaces, at which the refraction may be supposed to take place (Nat. Phil. II.

p. 81:) and $u = \frac{s}{1+px}$, s being a constant quantity. The angular refraction at each point will obviously be directly as the elementary change of this perpendicular, and inversely as the distance v from the point of incidence; whence the fluxion of the refraction will be $\frac{du}{v} = dr$, as is already well known.

3. For the fluent of this expression, which cannot be directly integrated, we may obtain a converging series by means of the TAYLORIAN theorem; but we must make the fluxion of the refraction constant, and that of the density variable; so that the equation will be $u = \frac{dv}{dr} \cdot r + \frac{d^2v}{dr^2} \cdot \frac{r^2}{2} + \frac{d^3v}{dr^3} \cdot \frac{r^3}{6} + \dots$, u being the initial value of u , when $r = 0$. Now the whole variation, of which u is capable, while z decreases from 1 to 0, extends from $\frac{s}{1+p}$ to s ; or, since p is very small, from $s - ps$ to s ; and dr being $= \frac{du}{v}$, we have

the equation $ps = vr + \frac{dv}{dr} \cdot \frac{r^2}{2} + \dots$. But $v = \sqrt{(x^2 - u^2)}$, $dv = \frac{xdx - udu}{v}$, and $\frac{dv}{dr} = \frac{x}{v} \cdot \frac{dx}{dr} - u$;

and dx being $= -\frac{dy}{mz}$, and $du = -psdx$, $\frac{dx}{dr} = \frac{v}{mpsz} \cdot \frac{dy}{dz}$.

4. We must now determine the value of the density z , which, when the temperature is uniform, becomes simply y ; but for which we must find some other function of y , including the variation of temperature; and we may adopt, for this purpose, the hypothesis lately advanced by Professor LEBLIE, in the article Climate of the Encyclopædia Britannica, and suppose the density to be augmented, by the effect of cold, in the proportion of

1 to $1+n \left(\frac{1}{z} - z \right)$, n being somewhat less than $\frac{1}{10}$; and since the density is as the pressure and the comparative specific gravity conjointly, we have $z = y \left(1+n \left[\frac{1}{z} - z \right] \right)$, $\frac{z}{y} = 1 + \frac{n}{z} - nz$, $d \frac{z}{y} = \frac{dz}{y} -$

$\frac{zdy}{yy} = -\frac{ndz}{xz} - ndx$, and $\frac{dy}{dz} = \frac{y}{z} + \frac{nyy}{z^3} + \frac{nyy}{z}$; consequently $\frac{dz}{dr} = \frac{v}{mpsz} \left(\frac{y}{z} + \frac{nyy}{z^3} + \frac{nyy}{z} \right)$

and $\frac{dv}{dr} = \frac{xy}{mpszx} + \frac{nxyy}{mpszx} + \frac{nxyy}{mpszx} - u$. We may proceed to take the next fluxion with respect to y , z , and v , the variations of u and x being comparatively inconsiderable: so that if we call

$\frac{dv}{dr} = X + Y + Z - s$, its fluxion will be $X \left(\frac{dy}{ydr} - \frac{zdx}{zdr} \right) + Y \left(\frac{zdy}{ydr} - \frac{zdx}{zdr} \right) + Z \left(\frac{zdy}{ydr} - \frac{4dx}{zdr} \right)$;

but since $\frac{dy}{ydr} = \frac{-v}{psz} - \frac{2vvy}{psz^3} - \frac{2vvy}{psz}$, and $\frac{dz}{zdr} = \frac{-v}{psz}$, we have $\frac{d^2v}{dr^2} = X \left(\frac{v}{psz} - \frac{2vvy}{psz^3} - \frac{2vvy}{psz} \right) + Y$
 $\left(-\frac{2vvy}{psz^3} - \frac{2vvy}{psz} \right) + Z \left(\frac{2v}{psz} - \frac{4vvy}{psz^3} - \frac{4vvy}{psz} \right) = \frac{vx}{mp^2s^2} \left(\frac{y}{z^3} - \frac{2ny^2}{z^3} - \frac{2ny^2}{z^3} - \frac{2n^2y^3}{z^3} + \right.$

$\frac{2ny^2}{z^5} - \frac{4n^2y^2}{z^7} - \frac{4n^2y^2}{z^5}$, or, initially $= \frac{v}{mp^2s^2}(1-2n-2n^2-2n^2-4n^2-4n^2) = \frac{1-2n-12nn}{mp^2s^2}v$. In the next place, calling this fluxion H (K-L-M-N-P-Q) we obtain, for the fourth, H (K-L-M-N-P-Q) $\frac{dv}{vdr} + HK \left(\frac{dy}{ydr} - \frac{3dz}{zdr} \right) - HL \left(\frac{2dy}{ydr} - \frac{3dz}{zdr} \right) - HM \left(\frac{3dy}{ydr} - \frac{5dz}{zdr} \right) - HN \left(\frac{3dy}{ydr} - \frac{3dz}{zdr} \right) - HP \left(\frac{2dy}{ydr} - \frac{7dz}{zdr} \right) - HQ \left(\frac{2dy}{ydr} - \frac{5dz}{zdr} \right) = H(K-L-M-N-P-Q) \left(\frac{x}{mps} \left(\frac{y}{zz} + \frac{nyy}{zz} + \frac{nyy}{z^4} \right) - u \right) + HK \frac{v}{ps} \left(\frac{2}{z} - \frac{2ny}{z^3} - \frac{2ny}{z} \right) - HL \frac{v}{ps} \left(\frac{1}{z} - \frac{4ny}{z^3} - \frac{4ny}{z} \right) - HM \frac{v}{ps} \left(\frac{2}{z} - \frac{6ny}{z^3} - \frac{6ny}{z} \right) - HN \frac{v}{ps} \left(-\frac{6ny}{z^3} - \frac{6ny}{z} \right) - HP \frac{v}{ps} \left(\frac{5}{z} - \frac{4ny}{z^3} - \frac{4ny}{z} \right) - HQ \frac{v}{ps} \left(\frac{3}{z} - \frac{4ny}{z^3} - \frac{4ny}{z} \right) = \frac{x^2}{m^2p^3s^3} \left(\frac{y^2}{z^5} - \frac{2ny^2}{z^5} - \frac{2n^2y^4}{z^7} - \frac{2n^2y^4}{z^5} - \frac{4n^2y^3}{z^7} - \frac{4n^2y^3}{z^5} + \frac{y^3}{z^5} - \frac{2n^2y^4}{z^7} - \frac{2n^2y^4}{z^5} - \frac{4n^2y^3}{z^7} - \frac{4n^2y^3}{z^5} \right) - \frac{x}{mp^2s} \left(\frac{y}{z^3} - \frac{2ny^2}{z^3} - \frac{2n^2y^3}{z^5} - \frac{2n^2y^3}{z^3} - \frac{4n^2y^2}{z^7} - \frac{4n^2y^2}{z^5} \right) + \frac{v^2x}{m^2p^3s^3} \left(\frac{2y}{z^4} - \frac{2ny^2}{z^6} - \frac{2ny^2}{z^4} - \frac{2ny^2}{z^4} + \frac{8n^2y^2}{z^6} + \frac{8n^2y^2}{z^4} - \frac{4n^2y^3}{z^6} + \frac{12n^2y^4}{z^8} + \frac{12n^2y^4}{z^6} + \frac{12n^2y^4}{z^6} + \frac{12n^2y^4}{z^4} - \frac{20n^2y^2}{z^8} + \frac{16n^2y^3}{z^{10}} + \frac{16n^2y^3}{z^8} - \frac{12n^2y^2}{z^6} + \frac{16n^2y^3}{z^8} + \frac{16n^2y^3}{z^6} \right). It will be unnecessary to continue the whole series any further; but it will be satisfactory to obtain that part of the sixth term, which is independent of v ; and for this purpose we must take the fluxion of the first part with respect to y and z , and then with respect to v ; and that of the second twice with respect to v only; and it will be sufficient in this case to employ the initial values of $\frac{dy}{dr}$,$

$\frac{dz}{dr}$, and $\frac{dv}{dr}$, which are $\frac{-v(1+4n)}{ps}$, $\frac{-v}{ps}$, and $\frac{1+2n}{mps} - s$; and calling $1+4n = k$, the part required will be $\left(\frac{1}{m^2p^4s^4} (-2k+5+6kn-10n+8kn^2-14n^2+8kn^2-10n^2+12kn^2-36n^2+12kn^2-28n^2-3kn+5n+8kn^2-10n^2+10kn^3-14n^3+10kn^3-10n^3+16kn^3-36n^3+16kn^2-28n^2-3kn+7n+8kn^2-14n^2+10kn^3-18n^3+10kn^3-14n^3+16kn^3-44n^3+16kn^2-36n^3) - \frac{1}{mp^3s^2} [-k+3+4kn-6n+6kn^2-10n^2+6kn^2-6n^2+8kn^2-28n^2+8kn^2-20n^2] \right) \left(\frac{1+2n}{mps} - s \right) + 2 \left(\frac{1+2n}{mps} - s \right)^2 \frac{1}{mp^3s^3} (2-2n-2n-2n+8n^2+8n^2-4n^2+12n^3+12n^3+12n^3+12n^3-20n^2+16n^3+16n^3-12n^2+16n^3+16n^3) = \left(\frac{1}{m^2p^4s^4} (3-6n-56n^2+128n^3+416n^4) - \frac{1}{mp^3s^2} [2-6n-20n^2+112n^3] \right) \left(\frac{1+2n}{mps} - s \right) + 2 \left(\frac{1+2n}{mps} - s \right)^2 \frac{1}{mp^3s^3} (2-6n-20n^2+112n^3) + \dots$

6. The whole equation becomes therefore ultimately $ps = vr + \left(\frac{1+2n}{2mps} - \frac{s}{2} \right) r^2 + \frac{1-2n-12nn}{6mp^2s^2} vr^3 + \left(\frac{1-16n^2-24n^3}{24m^2p^3s^3} - \frac{1-2n-12nn}{24mp^2s} + \frac{2-6n-20n^2+112n^3}{24mp^3s^3} v^2 \right) r^4 + \dots + \left(\left[\frac{3-6n-56n^2+128n^3+416n^4}{720m^2p^4s^4} - \frac{2-6n-20n^2+112n^3}{720mp^3s^2} \right] \left(\frac{1+2n}{mps} - s \right) + 2 \left(\frac{1+2n}{mps} - s \right)^2 \frac{2-6n-20n^2+112n^3}{720mp^3s^3} + \dots \right) r^6 + \dots$ We also obtain, for finding, on this hypothesis, the height x , corresponding to the pressure y and the density z , the expression $mx = m = 1 - \frac{y}{z} + \frac{n}{q} \ln \frac{2xz + qy(1-x)}{2xz - qy(1-x)}$; y being $= \frac{xz}{x+n-nxz}$, and $q^2 = 1+4n^2$. But the utility of the TAYLORIAN theorem, thus applied, in obtaining a series, is not confined to Professor LAGRANGE'S hypothesis: it is equally well adapted to that of LAPLACE, or to any other admissible supposition respecting the distribution of temperatures: and we may therefore employ it in examining the comparative accuracy of the results of these different hypotheses.

7. Now if we take for n the value $\frac{45}{500} = .09$, corresponding to the multiplier 45, employed by Mr. LESLIE, the refractions in the immediate neighbourhood of the horizon will become too great by about $1'$; a difference by far too considerable to be attributed to the errors of observation only; and we must infer, that the law of temperature, obtained from the height of the line of congelation, is not correctly true, if applied to elevations remote from the earth's surface. Professor BESSLER's approximation is also found to make the horizontal refraction too great. Mr. LAPLACE's formula, which affords a very correct determination of the refraction, is said to agree sufficiently well with direct observation also; but in fact this formula gives a depression considerably greater than was observed by GAY LUSSAC, in the only case which is adduced in its support; and the progressive depression follows a law which appears to be opposite to that of nature, the temperature varying less rapidly at greater than at smaller heights, while the observations of HUMBOLDT and others seem to prove that in nature they vary more rapidly. Notwithstanding, therefore, the ingenuity, and even utility of Mr. LAPLACE's formula, it can only be considered as an optical hypothesis, and we are equally at liberty to employ any other hypothesis which represents the results with equal accuracy; or even to correct our formulas by comparison with astronomical observations only, without assigning the precise law of temperature implied by them. The theory will however afford us some general indications for this purpose; showing, for example, that the coefficient of the second term cannot be smaller than $\frac{1}{2m\phi s} - \frac{1}{2}s$, whatever positive value we may attribute to n ; and if we adjust the second and fourth coefficients, so as to represent the refractions near the zenith and at the horizon, without regarding the value of the subsequent terms, we shall obtain the third, by dividing the fourth by half of the second; since that part of the fourth coefficient, which occurs in the case of horizontal refraction, is always derived from the third by taking the fluxion with respect to v only, and is therefore found by multiplying the third by $\frac{dv}{4vdr}$, whatever the relations of the other quantities concerned may be.

8. On every supposition, the coefficient of the first term must be $\frac{v}{s}$, and that of the second must not greatly differ from $\frac{3}{s} - \frac{s}{2}$. The third coefficient, on the hypothesis of a law analogous to Mr. LESLIE's, will be $1500 \frac{v}{s}$; if we suppose the temperature to vary more uniformly, and make $z = y(1 + tx - t)$, the number will become 1900; or, taking $z = yx^m$, 2200, m being 766, and t 176: and Mr. LAPLACE's formula will of course give a value still larger. In fact the result of observation is represented with sufficient accuracy by the equation $.0002825 = v \frac{r}{s} + (2.5 + .5v^2) \frac{r^2}{s^2} + 3400 v \frac{r^3}{s^3} + 3400 (1.25 + .25v^2) \frac{r^4}{s^4}$, the barometer standing at 30 inches, and the thermometer at 50° : and this formula appears to be at least as accurate as the French tables. We have, for example:

Altitude	Refr.	Conn. d. T.	Altitude	Refr.	Conn. d. T.
0			0		
0 0	33 52	33 52	20 0	2 39	2 40
5 0	9 57	9 56	30 0	1 41	1 41
10 0	5 21	5 21	45 0	58.15	58.3

The difference is somewhat greater a few degrees above the horizon; thus at $20^\circ 17' 50''$, this formula makes the refraction $17' 16''$, the French tables $17' 4''$, BRADLEY's $17' 30''$, and Dr. BRINKLEY's observations reduced, $17' 9''$: but in such cases we can scarcely expect a greater degree of accuracy.

9. The terrestrial refraction may be most easily determined by an immediate comparison with the angle subtended at the earth's centre, the fluxion of which is $\frac{vdx}{vx}$, and $\frac{vdx}{vxdx}$ is initially the first part of the coefficient of the second term of the series already obtained, and is equal to 6; so that this angle, while it remains small, is six times the refraction: commonly, however, the refraction in the neighbourhood of the earth's surface is somewhat less than in this proportion.

10. The effects of barometrical and thermometrical changes may be deduced from the fluxion of the equation; if we make m , ϕ , and n , or rather t , vary: and for this purpose it will be convenient to employ the form

$ps = vr + \left(\frac{1}{2(m-t)} + \frac{s}{2} \right) r^2$, the value of the fraction, if we neglect the subsequent terms, becoming 3.41; and this expression is sufficiently accurate for calculating the whole refraction, except for altitudes of a few degrees. Now the fluxion of $p = v \frac{r}{s} + \left(\frac{1}{2(m-t)} - \frac{ss}{2} \right) \frac{rr}{ss}$, which we may call $p = v \frac{r}{s} + \left(\frac{1}{w} - \frac{ss}{2} \right) \frac{rr}{ss}$, is $dp = \left(\frac{v}{s} + \left(\frac{1}{w} - \frac{ss}{2} \right) \frac{2r}{ss} \right) dr - \frac{rr}{ssw} \left(\frac{dm-dt}{m-t} + \frac{dp}{p} \right)$, the coefficient of dr being equal to $\frac{2p}{r} - \frac{v}{s}$; and $\left(2p - \frac{rv}{s} \right) \frac{dr}{r} = \left(p + \frac{rr}{ssw} \right) \frac{dp}{p} + \frac{rr}{ssw} \left(\frac{dm-dt}{m-t} \right)$; $\frac{1}{w}$ being 3.41, and $m-t$, on this supposition, 519. The proportional variation of p , or $\frac{dp}{p}$, will be $\frac{1}{519}$ for every degree that the thermometer varies from 50° ; and $\frac{dm}{m}$ being also $\frac{1}{519}$, $\frac{dm}{m-t}$ will be $\frac{766}{519 \times 500} = .003$. The variation of t can only be determined from conjecture; but supposing the alteration of temperature to cease at the height of about 4 miles, it must increase, with every degree that the thermometer rises at the earth's surface, about $\frac{1}{138}$, and $\frac{dt}{t}$ being $\frac{1}{138}$, $\frac{dt}{m-t}$ will be $\frac{247}{519 \times 120} = .004$. The alterations of the barometer will affect p only, $\frac{dp}{p}$ being $\frac{1}{36}$ for every inch above or below 30. It is evident, since $m = \frac{3958 \times 5280 \times 12}{13.57 bd}$, b being the height of the barometer, and d the bulk of air compared to that of water, that m must diminish, as well as p , when the temperature increases; and the correction for t being subtractive, the three variations will co-operate in their effects; but the proportion will be somewhat different from that of the simple densities. If we preferred the expression derived from Professor LESLIE's hypothesis, we should merely have to substitute $\frac{2dn}{1+2n}$ for $\frac{dt}{m-t}$, and the variation depending on the law of temperature would become about $\frac{2}{3}$ as great. It must however be limited to such changes as affect the lower regions of the atmosphere only, its "argument" being the deviation from the mean temperature of the latitude; but even in this form it cannot be satisfactorily applied to the observations at present existing; although it appears to be amply sufficient to explain the irregularities of terrestrial refraction, as well as the uncommon increase of horizontal refraction in very cold countries; and we may even derive from all these considerations a correction of at least half a second, or perhaps of a whole second, for the sun's altitude at the winter solstice, tending to remove the discordance, which has so often been found, in the results of some of the most accurate observations of the obliquity of the ecliptic.

T. Y.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

MDCCCXIX.

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METEOROLOGICAL JOURNAL

for January, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
an.	1	8 0	28	41	30,01	26	NW	1	Foggy.
		2 0	33	48	30,02	33	NE	1	Fine.
	2	8 0	33	41	30,01	28	NW	1	Cloudy.
		2 0	34	47	29,96	36	E	1	Cloudy, thick weather.
	3	8 0	32	48	29,61	31	SE	1	Snow.
		2 0	35	47	29,57	35	SW	1	Cloudy.
	4	8 0	41	43	29,48	35	SSW	1,2	Rain.
		2 0	43	45	29,51	45	SSW	1	Cloudy.
	5	8 0	42	43	29,52	37	SSW	2,3	Rain.
		2 0	43	48	29,42	45	SW	1,2	Rain.
	6	8 0	34	44	29,88	34	NW	1	Cloudy.
		2 0	41	48	30,09	41	WNW	1	Fair.
	7	8 0	39	45	30,15	34	SSW	1	Foggy.
		2 0	45	49	30,02	47	SW	1	Cloudy.
	8	8 0	38	47	30,06	37	NNW	1	Cloudy.
		2 0	42	52	30,19	43	NW	1	Rain.
	9	8 0	39	49	30,16	34	S	1	Fair.
		2 0	49	55	29,95	49	W	1	Rain.
	10	8 0	48	50	29,83	46	W	1	Cloudy.
		2 0	50	55	29,81	53	W	1	Rain.
	11	8 0	47	51	29,79	48	W	1	Cloudy.
		2 0	50	52	29,72	51	W	1	Cloudy.
	12	8 0	40	48	29,84	38	NNW	1	Fair.
		2 0	44	53	30,08	44	WbyS	1	Cloudy.
	13	8 0	48	50	29,84	41	W	1,2	Cloudy.
		2 0	50	54	29,80	52	W	2	Cloudy.
	14	8 0	47	54	29,84	45	NW	1	Cloudy.
		2 0	45	55	29,87	52	SW	1	Fair.
	15	8 0	50	54	29,69	42	W	2,3	Cloudy.
		2 0	53	58	29,67	55	W	1,2	Rain.
	16	8 0	47	55	29,88	47	W	1	Cloudy.
		2 0	49	59	29,81	52	W	1	Rain.

Rain this Month 1,461 Inches.

METEOROLOGICAL JOURNAL

for January, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Jan. 17	8	0	40	56	29.88	39	NE	1	Fair. } A remarkably fine day for the season.
	2	0	43	57	29.82	43	NE	1	
18	8	0	39	53	29.92	37½	WNW	1	
	2	0	41	54	30.02	43	E	1	Cloudy.
19	8	0	34	49	30.46	32	W	1	Fine.
	2	0	42	57	30.50	42	W	1	Fine.
20	8	0	36	50	30.37	34	WSW	1	Fine.
	2	0	42	57	30.25	43	S	1	Fine.
21	8	0	42	51	29.91	36	W	1	Rain.
	2	0	45	57	30.07	46	WbyN	1	Fine.
22	8	0	39	51	30.00	34	SW	1,2	Fine.
	2	0	44	56	29.80	46	SW	1,2	Cloudy. } A violent squall of wind and hail at 10 p. m.
23	8	0	36	51	29.72	34	WSW	1	Fine.
	2	0	41	54	29.59	43	W	1	Cloudy.
24	8	0	40	51	29.51	35	W	1	Rain.
	2	0	42	57	29.51	45	NW	1	Fine.
25	8	0	36	49	29.98	33	NW	1	Hazy, but fine.
	2	0	43	49	30.00	43	W	1	Fair.
26	8	0	47	49	29.89	41	W	1	Cloudy.
	2	0	49	54	29.70	50	W	1	Cloudy.
27	8	0	40	51	29.94	36	W	1	Fine.
	2	0	44	54	29.83	55	SW	1	Cloudy.
28	8	0	41	52	29.58	39	W	1	Cloudy.
	2	0	43	57	29.67	45	W	1	Cloudy.
29	8	0	37	49	29.71	33	W	1	Fair.
	2	0	41	55	29.54	44	S	1	Fine.
30	8	0	41	52	29.02	40	SW	1,2	Rain. } A violent storm of wind from 12-5.
	2	0	45	57	28.84	46	W	1	Cloudy.
31	8	0	37	51	29.30	35	W	1	Fine.
	2	0	39	57	29.43	39	W	1	Fine.

Rain this Month 1,46½ Inches.

METEOROLOGICAL JOURNAL
for February, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Feb. 1	8	0	34	49	29,18	34	W	1	Rain and snow.
	2	0	42	52	29,14	42	WNW		Fine.
2	8	0	32	47	28,97	29	SE	1	Hazy.
	2	0	33	53	28,96	33	N	1	Cloudy.
3	8	0	31	47	29,14	27	E	1	Fine.
	2	0	37	53	29,14	39	WNW	1	Rain.
4	8	0	32	46	29,14	28	NE	1	Cloudy.
	2	0	34	49	29,11	35	N	1	Cloudy.
5	8	0	31	45	29,54	28	SW	1	Cloudy.
	2	0	44	53	29,69	44	W	1	Cloudy.
6	8	0	33	49	29,97	31		1	Thick fog.
	2	0	40	54	29,98	38	E	1	Hazy.
7	8	0	31	48	29,08	29		1	Thick fog.
	2	0	31	53	29,09	31	S	1	Cloudy and hazy.
8	8	0	27	45	30,03	26	Nbve	0,1	Cloudy and hazy.
	2	0	30	45	29,99	31	W	1	Hazy.
9	8	0	26	42	30,03	26	W	1	Cloudy and hazy.
	2	0	32	59	30,04	33	S	1	Cloudy and hazy.
10	8	0	28	43	30,01	26	W	1	Thick fog.
	2	0	33	48	30,05	35	NE	1	Cloudy.
11	8	0	33	45	30,18	33	E	1	Cloudy and rather hazy.
	2	0	37	52	30,22	37	SE	1	Hazy.
12	8	0	31	44	30,20	31	W	1	Cloudy.
	2	0	36	51	30,18	36	S	1	Cloudy.
13	8	0	34	46	30,11	33	SE	1	Cloudy.
	2	0	39	54	30,01	41	SE	1	Fine.
14	8	0	33	47	29,88	29	SE	1	Fine.
	2	0	38	47	29,82	39	S	1	Fair.

Rain this Month 0,727 Inches.

METEOROLOGICAL JOURNAL
for February, 1818.

1818	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H. M.	°	°	Inches.		Points.	Str.	
Feb. 15	8 0	33	47	29.90	30	ESE	1	Hazy.
	2 0	40	49	29.91	42	SE.	1	Rain.
16	8 0	39	44	29.91	33	SE	1	Cloudy.
	2 0	46	51	29.94	49	SSW	1	Cloudy.
17	8 0	44	49	29.99	42	SE	1	Cloudy.
	2 0	48	61	29.96½	51	S	1	Fine.
18	8 0	47	54	29.95	44	S	1,2	Rain.
	2 0	48	59	29.93	49	SSW	2	Rain.
19	8 0	42	52	30.09	41	W	1	Foggy.
	2 0	50	61	29.89	52	SbyE	1	Rain.
20	8 0	41	57	30.06	34	S	1	Fine.
	2 0	46	62	30.04	49	N	1	Fine.
21	8 0	44	55	29.70	36	S	2	Cloudy.
	2 0	46	57	29.37	47	NW	1	Rain.
22	8 0	40	52	29.29	39	NW	1	Rain.
	2 0	40	51	29.05	41	SE	1	Rain.
23	8 0	34	47	29.78	32	N	1	Fine.
	2 0	44	57	29.83	44	S	1	Cloudy.
24	8 0	37	49	29.78	34½	S	1,2	Fine.
	2 0	44	57	29.88	46	W	1	Fair.
25	8 0	48	53	29.59	42	W	1,2	Cloudy.
	2 0	51	60	29.58	52	SW	1,2	Fair.
26	8 0	40	52	29.42	39	W	1	Cloudy.
	2 0	42	60	29.70	56	N	1	Cloudy.
27	8 0	38	52	29.56	34	S	1	Rain.
	2 0	48	62	29.35	50	W	1	Fine.
28	8 0	40	53	29.67	37	WSW	1	Cloudy.
	2 0	46	59	29.61	49	SW	1	Cloudy.

Rain this Month 0.727 Inches.

A fall of snow at 5 p.m.
Barom. 29.27 at 11 p.m.

METEOROLOGICAL JOURNAL

for March, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.	
	H.	M.	°	°	Inches.		Points.	Str.		
Mar.	1	7	0	39	55	29.69	38	SW	1	Fair.
		2	0	44	59	29.63	51	W	1	Fair.
	2	7	0	40	51	29.70	37	W	1	Fair.
		2	0	45	58	29.60	50	W	1	Rain.
	3	7	0	41	53	29.78	37	S	1	Fine.
		2	0	49	57	29.68	50	SW	1,2	Cloudy.
	4	7	0	37	52	29.39	35	W	1	Fine.
		2	0	43	55	29.22	48	S	1	Cloudy.
	5	7	0	43	51	28.88	42	S	2	Cloudy.
		2	0	43	61	29.01	47	WNW	1	Fair.
	6	7	0	41	53	29.29	38	SW	1	Fair.
		2	0	45	62	29.41	49	W	1,2	Cloudy.
	7	7	0	43	52	29.24	36	S	2,3	Rain.
		2	0	44	57	28.81	50	W	2	Cloudy.
	8	7	0	40	51	28.95	39	W	1,2	Fine.
		2	0	47	54	29.11	52	W	2	Fine.
	9	7	0	36	48	29.39	34	SW	1	Fair.
		2	0	42	54	29.42	43	WNW	1	Cloudy.
	10	7	0	33	49	29.36	31	W	1	Cloudy.
		2	0	38	57	29.34	41	NW	1	Cloudy.
	11	7	0	37	49	29.48	33	W	1	Fair.
		2	0	44	57	29.51	44	W	1	Cloudy.
	12	7	0	35	49	29.87	34	W	1	Cloudy.
		2	0	45	57	29.92	46	W	1,2	Cloudy.
	13	7	0	37	51	29.28	35	NE	1	Cloudy.
		2	0	42	58	29.66	44	N	1	Fine.
	14	7	0	35	50	29.85	32	E	1	Fine.
		2	0	44	58	29.88	46	W	1	Cloudy.
	15	7	0	41	50	29.54	40	SW	1,2	Cloudy.
		2	0	45	46	29.39	49	SSW	1	Hazy.
	16	7	0	40	48	29.47	36	W	1,2	Cloudy.
		2	0	47	58	29.56	48	NW	1	Fine.

Rain this Month 0.986 Inches.

A violent gale between 10 and 12 last night, baro. 28.61 at 11 30 p.m.

Rain this Month 0.986 Inches.

A violent gale between 10 and 12 last night, baro. 28.61 at 11 30 p.m.

METEOROLOGICAL JOURNAL

for March, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Mar. 17	7	0	45	51	29.72	40.	W	1	Rain.
	2	0	50	59	29.93	51	N	1	Cloudy.
18	7	0	41	53	30.09	40	W	1	Cloudy.
	2	0	51	60	30.05	53	WNW	1	Cloudy.
19	7	0	46	55	30.01	46	W	1	Cloudy.
	2	0	50	60	29.94	53	WSW	1	Cloudy.
20	7	0	48	56	29.69	48	NW	1	Cloudy.
	2	0	51	68	29.86	52	NW	1	Fine.
21	7	0	40	50	29.97	35	W	1	Fair.
	2	0	46	62	29.88	52	W	1,2	Rain and squally.
22	7	0	46	54	29.66	40	W	1	Rain.
	2	0	49	57	29.68	54	NW	1	Fine, showery.
23	7	0	44	53	29.28	47	NE	1	Rain and squally.
	2	0	47	63	29.53	52	W	1	Fine, showery.
24	7	0	40	53	29.68	38	WSW	1	Cloudy.
	2	0	48	57	29.68	50	WNW	1	Showery.
25	7	0	39	52	29.56	37	W	2	Cloudy.
	2	0	43	58	29.64	49	N	1,2	Cloudy.
26	7	0	42	51	29.57	34	WNW	1	Rain.
	2	0	40	57	29.22	43	NE	1	Rain.
27	7	0	38	51	30.01	34	W	1	Cloudy.
	2	0	44	48	30.17	46	NW	1	Fine.
28	7	0	39	52	30.33	35	S	1	Cloudy.
	2	0	46	58	30.30	50	S	1	Cloudy.
29	7	0	42	51	30.21	38	SE	1	Fine.
	2	0	46	53	30.14	52	S	1	Fine.
30	7	0	41	49	30.17	37	S	1	Fine.
	2	0	50	58	30.24	55	W	1	Cloudy.
31	7	0	40	52	30.34	38	SE	1	Fine.
	2	0	46	59	30.31	48	E	1	Cloudy.

Rain this Month 0.986 Inches.

METEOROLOGICAL JOURNAL

for April, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
April 1	7	0	43	52	30.26	38	E	1	Fine.
	2	0	44	59	30.22	49	NW	1	Fine.
2	7	0	41	52	30.31	39	N	1	Cloudy.
	2	0	45	59	30.33	47	NE	1	Fine.
3	7	0	41	52	30.36	39	E	1	Cloudy.
	2	0	46	61	30.38	48	SE	1	Cloudy.
4	7	0	40	52	30.40	35	S	1	Fine.
	2	0	46	62	30.37	48	SE	1	Fine.
5	7	0	40	52	30.10	33	SSE	1	Fair.
	2	0	43	55	30.07	54	NW	1	Fine.
6	7	0	48	54	29.31	42	SW	1,2	Cloudy, rain in the night.
	2	0	50	57	29.32	52	N	1,2	Cloudy.
7	7	0	40	51	29.72	39	SSE	1	Rain.
	2	0	49	58	29.56	50	SW	1	Rain.
8	7	0	53	56	29.49	50	S	1	Cloudy.
	2	0	49	59	29.45	62	NE	1	Rain.
9	7	0	53	56	29.37	50	S	1	Fair.
	2	0	54	64	29.38	57	W	1	Cloudy.
10	7	0	47	58	29.61	44	S	1	Rain.
	2	0	52	65	29.50	55	S	1	Rain.
11	7	0	46	58	29.27	46	S	1	Fine.
	2	0	44	57	29.45	59	SSW	1	Rain.
12	7	0	41	53	29.93	46	NNW	1	Fine.
	2	0	47	53	30.03	51	SW	1	Fine.
13	7	0	38	51	30.08	35	S by W	1	Cloudy.
	2	0	48	60	29.96	52	SW	1	Fine.
14	7	0	43	52	29.71	40	SE	1	Hazy.
	2	0	51	60	29.72	56	SE	1	Cloudy.
15	7	0	44	54	29.77	38	E	1	Fine, but rather hazy.
	2	0	51	63	29.70	55	SE	1	Fair.

Rain this Month 1,791 Inches

METEOROLOGICAL JOURNAL

for April, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Siv's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Apr. 16	7	0	45	55	29.44	41	SE	1	Fine.
	2	0	54	63	29.54	56	SE	1	Cloudy.
17	7	0	49	57	29.28	46	E	1	Rain.
	2	0	52	62	29.62	54	E	1	Cloudy.
18	7	0	45	55	29.33	44	E	1	Cloudy.
	2	0	51	68	29.45	54	ESE	1	Fine.
19	7	0	43	55	29.71	43	NE	1	Cloudy.
	2	0	48	56	29.76	50	E	1	Cloudy.
20	7	0	40	53	29.82	35	E	1	Fine, rather hazy.
	2	0	48	58	29.81	50	E	1	Cloudy.
21	7	0	44	54	29.71	35	E	1	Cloudy.
	2	0	52	63	29.70	53	SE	1	Cloudy.
22	7	0	49	55	29.71	45	E	1	Cloudy and hazy.
	2	0	53	59	29.63	56	E	1,2	Rain.
23	7	0	48	54	29.56	45	E	1,2	Rain.
	2	0	45	61	29.48	50	E	1	Thick dark foggy weather.
24	7	0	46	58	29.30	44	ESE	1	Rain.
	2	0	51	62	29.33	53	E	1	Cloudy.
25	7	0	50	58	29.13	43	S	1	Rain.
	2	0	64	63	29.19	55	SW	1	Fair.
26	7	0	52	57	29.42	43	NW	1	Cloudy.
	2	0	61	61	29.47	65	SW	1	Fine.
27	7	0	55	59	29.42	53	E	1,2	Cloudy, but fine.
	2	0	60	65	29.57	64	SW	1	Fine.
28	7	0	50	59	29.83	49	S by E	1	Fair.
	2	0	58	63	29.90	62	SW	1	Fair.
29	7	0	50	61	30.00	42	W	1	Fair.
	2	0	59	65	29.95	64	E	1	Fine.
30	7	0	51	59	29.76	45	E	1	Cloudy and hazy.
	2	0	53	64	29.61	54	E	1	Rain.

Rain this Month 1.791 Inches.

METEOROLOGICAL JOURNAL

for May, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Siv's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
May	1	7 0	53	60	29.75	51	N	1	Fine.
		2 0	69	68	29.85	62	W	1	Fine, but showery.
	2	7 0	50	60	29.90	46	E	1	Cloudy.
		2 0	61	66	29.81	64	E	1	Cloudy.
	3	7 0	53	60	29.55	49	E	1	Rain.
		2 0	60	63	29.47	64	E	1	Fine.
	4	7 0	51	60	29.52	49	W	1	Fine.
		2 0	61	64	29.53	66 $\frac{1}{2}$	W	1	Fine.
	5	7 0	52	60	29.49	49	N	1	Fine.
		2 0	60	63	29.43	64	E	1	Cloudy.
	6	7 0	52	59	29.35	51	NE	1	Cloudy.
		2 0	57	63	29.34	61	S	1	Cloudy.
	7	7 0	51	58	29.34	49 $\frac{1}{2}$	S	1,2	Cloudy.
		2 0	58	67	29.39 $\frac{1}{2}$	61	SSE	1	Cloudy.
	8	7 0	52	60	29.57	48	S	1	Fair.
		2 0	61	62	29.57	66	SSE	1	Cloudy.
	9	7 0	50	57	29.59 $\frac{1}{2}$	47	SW	1	Fine.
		2 0	57	61	29.63	61	SSE	1	Showery.
	10	7 0	50	58	29.86	47	W	1	Fine.
		2 0	59	60	29.87	64	S	1	Fine.
	11	7 0	56	58	29.74	48	S	1	Fine.
		2 0	58	61	29.65	66	E	1	Fine.
	12	7 0	54	59	29.63	52	W	1	Cloudy.
		2 0	61	60	29.65	65	W	1	Cloudy.
	13	7 0	50	58	29.49	48	SE	1	Rain.
		2 0	58	62	29.40	61	W	1,2	Fine, but showery.
	14	7 0	48	57	29.41	46	S	2	Rain.
		2 0	55	58	29.42	57	S	2	Squally rainy weather.
	15	7 0	52	58	29.52	47	W	1	Cloudy.
		2 0	58	53	29.52	62	N	1	Cloudy.
	16	7 0	53	61	29.56	48 $\frac{1}{2}$	E	1	Fine.
		2 0	61	60	29.59	66	SE	1	Cloudy.

Rain this Month 1.597 Inches.

METEOROLOGICAL JOURNAL

for May, 1818.

1818	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H. M.	°	°	Inches.		Points.	Str.	
May 17	7 0	54	58	29.68	52	SW	1	Cloudy.
	2 0	56	58	29.78	57	NNE	1	Rain.
18	7 0	54	58	29.87	48	NNE	1	Fair.
	2 0	64	66	29.89	69	E	1	Fair.
19	7 0	51	59	29.99	48	NE	1	Cloudy.
	2 0	57	60	30.00	58	N	1	Cloudy.
20	7 0	50	58	30.06	44	NE	1	Fine.
	2 0	64	59	30.06	65	E	1	Fair.
21	7 0	51	57	30.18	47	E	1	Cloudy.
	2 0	57	63	30.18	67	E	1	Fine.
22	7 0	50	58	30.23 $\frac{1}{2}$	41	E	1	Cloudy.
	2 0	57	63	30.24	60	E	1	Fair.
23	7 0	50	57	30.31	45	E	1	Cloudy, rather hazy.
	2 0	57	59	30.31	59	SE	1	Fine.
24	7 0	53	58	30.34	44	E	1	Cloudy.
	2 0	60	64	30.35	62	E	1	Fair.
25	7 0	53	58	30.33	49	E	1	Cloudy.
	2 0	60	64	30.29	64	EbyS	1	Fair.
26	7 0	59	59	30.27	47	N	1	Fair.
	2 0	62	65	30.27	68	E	1	Fair.
27	7 0	53	59	30.34	47	E	1	Cloudy.
	2 0	63	67	30.29	67 $\frac{1}{2}$	E	1	Fine.
28	7 0	53	58	30.22	44	N	1	Fine.
	2 0	64	66	30.16 $\frac{1}{2}$	68	NNE	1	Fair.
29	7 0	52	59	30.12	47	SE	1	Cloudy.
	2 0	59	62	30.12	63	SE	1	Cloudy.
30	7 0	51	59	30.14	45 $\frac{1}{2}$	ESE	1	Cloudy.
	2 0	55	60	30.11	57 $\frac{1}{2}$	NE	1	Cloudy.
31	7 0	52	58	30.00	45	SW	1	Cloudy.
	2 0	65	64	29.96	71	SE	1	Fair.

Rain this Month 1.597 Inches.

METEOROLOGICAL JOURNAL

for June, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
June 1	7	0	62	60	29.98	57	WSW	1	Cloudy.
	2	0	68	63	29.98	74	NNW	1	Cloudy.
2	7	0	64	63	30.02	60	SW	1	Fine.
	2	0	69	67	30.02	76	SW	1	Fine.
3	7	0	62	63	30.08	53	W	1	Fine.
	2	0	72	70	30.09	76	SW	1	Fair.
4	7	0	63	63	30.16	56	SE	1	Hazy and dark.
	2	0	73	73	30.18	77	S	1	Fair.
5	7	0	63	66	30.30	56	E	1	Cloudy.
	2	0	71	73	30.30 $\frac{1}{2}$	73	E	1	Fair.
6	7	0	63	66	30.34	55	E	1	Fair.
	2	0	70	73	30.31	75	E	1	Fair.
7	7	0	62	66	30.26	51	E	1	Fair.
	2	0	71	71	30.23	74 $\frac{1}{2}$	E	1	Fair.
8	7	0	64	66	30.26	56	E	1	Fine.
	2	0	69	73	30.28	74	E	1	Fair.
9	7	0	65	67	30.25 $\frac{1}{2}$	56	E	1	Fine.
	2	0	69	73	30.24	74	E	1	Fair.
10	7	0	65	67	30.23	53 $\frac{1}{2}$	E	1	Fine.
	2	0	71	74	30.22	78	E	1	Fair.
11	7	0	68	69	30.14 $\frac{1}{2}$	60	E	1	Fine.
	2	0	74	75	30.11	82	E	1	Fine.
12	7	0	68	69	30.04	57	E	1	Fine.
	2	0	78	76	29.99	82	E	1	Fine. Ther. 70 at 11 p.m.
13	7	0	67 $\frac{1}{2}$	70	29.94	63	W	1	Fair.
	2	0	75	77	29.92	85	E	1	Cloudy. { Thunder storm at 4 p.m.
14	7	0	66	70	29.94	65	W	1	Cloudy.
	2	0	69	70	29.99	73	N	1	Cloudy.
15	7	0	63	68	30.08	58	SW	1	Cloudy, but fine.
	2	0	71	72	30.03	75	W	1	Fine.
16	7	0	65	68 $\frac{1}{2}$	29.98 $\frac{1}{2}$	61	W	1	Fine.
	2	0	71	70	29.92	75	SSW	1	Cloudy.

Rain this Month 0.407 Inches.

METEOROLOGICAL JOURNAL

for June, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
June 17	7	0	64	67	29,82	54	SW	1	Cloudy.
	2	0	68	68	29,75	71	S	1,2	Cloudy.
18	7	0	59	66	29,75	57	E	1	Fine.
	2	0	68	67	29,74	72	N	1	Fine.
19	7	0	59	64	29,82	57	W	1	Cloudy.
	2	0	65	66	29,81	71	S	1,2	Cloudy.
20	7	0	58	64	29,65	54	SW	1,2	Fine.
	2	0	62	64	29,75	69	NW	1,2	Rain.
21	7	0	59	63	30,02	50	SW	1	Fine.
	2	0	65	64	29,99	70	S	1	Fine.
22	7	0	59	64	29,83	62	SW	1	Cloudy.
	2	0	66	66	29,71	70	SW	2	Cloudy.
23	7	0	57	62	29,82	59	SW	1	Fine.
	2	0	65	65	29,89	69	N	1	Fine.
24	7	0	61	63	29,92	56	N	1	Fair.
	2	0	70	67	30,02	74	W	1	Fine.
25	7	0	59	62	30,10	58	SW	1	Cloudy.
	2	0	72	68	30,04	75	NW	1	Fine.
26	7	0	64	67	30,05 $\frac{1}{2}$	60	W by N	1	Cloudy.
	2	0	70	67	30,07	76	NW	1	Cloudy.
27	7	0	64	67	29,96	59	SE	1	Fine.
	2	0	70	74	29,83	79	ESE	1,2	Cloudy, but fine.
28	7	0	63	74	29,78	58	NW	2	Cloudy.
	2	0	66	67	29,86	70	NW	1,2	Cloudy.
29	7	0	60	63	30,16	58	SW	1	Fine.
	2	0	71	69	30,19	76	NW	1	Cloudy.
30	7	0	63	66	30,27	60	W	1	Fine.
	2	0	70	72	30,24	80	W	1	Fine.

Rain this Month 0,407 Inches.

METEOROLOGICAL JOURNAL

for July, 1818.

1818	Time.		Therm.	Therm.	Barom.	Six's	Winds.		Weather.
	H.	M.	without.	within.	Inches.	Therm.	Points.	Str.	
Jan.	1	7 0	64	66	30,16	61	W	1	Fine.
		2 0	73	71	30,10	78	W	1	Cloudy.
	2	7 0	63	66	30,08	61	E	1	Fine.
		2 0	68	69	30,12	70	E	1	Cloudy.
	3	7 0	61	64	30,22	53	SW	1	Fine.
		2 0	71	70	30,18	75	W	1	Cloudy.
	4	7 0	64	67	30,09	64	NW	1	Cloudy.
		2 0	70	72	30,09	74	NW	1	Cloudy.
	5	7 0	65	67	30,08	59	N	1	Fine.
		2 0	70	72	30,06 $\frac{1}{2}$	75	N	1	Fine.
	6	7 0	63	68	30,11	60	S	1	Cloudy.
		2 0	69	73	30,09	73	E	1	Fine.
	7	7 0	61	67	30,04	55	E	1	Fine.
		2 0	71	72	29,93	83	E	1	Fine.
	8	7 0	63	68	29,88	59	W	1	Cloudy, but fine.
		2 0	69	71	29,91	72	NNW	1	Fine.
	9	7 0	62	67	30,11	55	N	1	Fine.
		2 0	68	69	30,13	74	SSE	1	Cloudy.
	10	7 0	64	67	30,12	54	W	1,2	Cloudy.
		2 0	69	69	30,08	74	S	1	Cloudy.
	11	7 0	65	65	30,19	60	NE	1	Cloudy.
		2 0	70	70	29,99	74	W	1	Fine.
	12	7 0	63	68	29,93	63	W	1	Rain.
		2 0	70	70	29,81	74	W	1	Cloudy.
	13	7 0	65	66	29,80	61	W	1	Cloudy.
		2 0	71	71	29,90	76	W	1	Fine.
	14	7 0	65	67	29,98	59	S	1	Fine.
		2 0	73	74	30,23	79	SW	1	Fine.
	15	7 0	68	68	30,30	62	E	1	Fine.
		2 0	74	74	30,30	81	W	1	Fine.
	16	7 0	68	68	30,26	64	W	1	Fine.
		2 0	80	76	30,23	85 $\frac{1}{2}$	N	1	Fine.

Rain this Month 0,361 Inches.

METEOROLOGICAL JOURNAL .

for July, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
July 17	7	0	68	69	30.18	68	E	1	Rain.
	2	0	72	73	30.15	75	E	1	Fine.
18	7	0	67	68	30.09	59	E	1	Cloudy.
	2	0	72	71	30.03	76	S	1	Cloudy.
19	7	0	68	69	29.95	61	S	1	Cloudy.
	2	0	75	76	29.92	81	E	1	Fine.
20	7	0	67	63	29.97½	63	N	1	Cloudy.
	2	0	72	76	29.97	77	NE	1	Fine.
21	7	0	64	68	29.98	62	W	1	Cloudy.
	2	0	72	72	29.97	76	NW	1	Fine.
22	7	0	67	69	30.08	64	W	1	Fine.
	2	0	75	75	30.10	81	SW	1	Cloudy.
23	7	0	69	70	30.13	65	SE	1	Fine.
	2	0	76	79	30.08	82	SE	1	Fair.
24	7	0	75	73	29.88	67	SE	1	Fair.
	2	0	78	81	29.86	89	SW	1	Fine.
25	7	0	68	74	29.84	69	NW	1	Fine.
	2	0	76	79	29.86	81	W	1	Fine.
26	7	0	68	70	29.88	65	E	1	Fine.
	2	0	75	78	29.84	81	SW	1	Fine.
27	7	0	71	70	29.84	69	SE	1	Cloudy, but fine.
	2	0	65	72	29.92	78	SE	1	Fair.
28	7	0	65	67	30.19	70	SW	1	Cloudy, but fine.
	2	0	71	72	30.24	72	W	1	Fair.
29	7	0	69	66	30.25	61	SW	1	Fair.
	2	0	73	70	30.22	71	NW	1	Fair.
30	7	0	69	69	30.16	70	WNW	1	Cloudy, but fine.
	2	0	75	72	30.14	76	W	1	Fine.
31	7	0	66	68	30.14	65	NW	1	Cloudy, but fine.
	2	0	64	66	30.09	70	NW	1	Rain.

Rain this Month 0.361 Inches.

METEOROLOGICAL JOURNAL

for August, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	o	o	Inches.		Points.	Str.	
Aug. 1	7	0	64	66	30.01	62	NW	1	Cloudy.
	2	0	68	69	30.03	73	NW	1	Fine.
2	7	0	67	69	30.14	65	W	1	Cloudy.
	2	0	68	69	30.17	69	NW	1	Fair.
3	7	0	67	65	30.04	66	NW	1	Cloudy.
	2	0	68	65	30.11	69	N	1	Fair.
4	7	0	63	65	30.14	64	N	1	Fair.
	2	0	69	70	30.08	76	NW	1	Fair.
5	7	0	67	66	30.10	60	ESE	1	Fair.
	2	0	76	76	30.11	84	ESE	1	Fine.
6	7	0	71	72	30.07	75	ENE	1	Fine.
	2	0	70	70	30.05	77	E	1	Fine.
7	7	0	68	67	30.07	65	SE	1	Fair.
	2	0	75	76	30.10	85	SW	1	Fine.
8	7	0	66	67	30.11	64	SW	1	Fair.
	2	0	65	67	30.16	74	SE	1	Fair.
9	7	0	61	62	30.10	60	S	1	Fine.
	2	0	62	68	30.17	69	S	1	Fine.
10	7	0	67	69	30.10	66	N	1	Fair.
	2	0	66	67	30.19	73	NE	1	Fair.
11	7	0	67	68	30.01	65	E	1	Rain.
	2	0	66	66	30.02	69	E	1	Cloudy.
12	7	0	65	66	30.07	68	NE	1	Cloudy.
	2	0	65	66	30.12	69	N	1	Cloudy.
13	7	0	63	65	30.15	61	N	1	Cloudy.
	2	0	70	71	30.14	79	SE	1	Fine.
14	7	0	71	72	30.07	69	N	1	Cloudy.
	2	0	69	70	30.14	76	NE	1	Fine.
15	7	0	66	66	30.25	61	E	1	Cloudy.
	2	0	68	68	30.24	73	SW	1	Fine.
16	7	0	66	67	30.17	68	S	1	Cloudy.
	2	0	65	64	30.19	73	SE	1	Fine.

Rain this Month 0.278 Inches.

METEOROLOGICAL JOURNAL

for August, 1818.

1818	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H. M.	°	°	Inches.		Points.	Str.	
Aug. 17	7 0	63	68	30,22	67	E	1	Fine.
	2 0	69	70	30,24	70	ESE	1	Fine.
18	7 0	67	69	30,08	63	E	1	Fine.
	2 0	65	67	29,94	73	NE	1	Fine.
19	7 0	56	58	29,91	56	NE	1	Cloudy.
	2 0	67	69	30,02	70	NE	1	Fine.
20	7 0	57	60	29,93	60	SE	1	Fair.
	2 0	65	68	30,05	75	SE	1	Fine.
21	7 0	59	61	29,91	59	SSE	1	Fair.
	2 0	67	68	30,02	69	SE	1,2	Fine.
22	7 0	60	61	30,01	58	ENE	1	Fine.
	2 0	65	66	30,12	68	NE	1	Fine.
23	7 0	53	54	30,02	55	N	1	Fair.
	2 0	63	64	30,28	72	NW	1,2	Fine.
24	7 0	59	60	30,10	61	N	1	Fine.
	2 0	62	63	30,18	67	N	1	Fine.
25	7 0	59	63	30,05	60	N	1	Fair.
	2 0	66	68	30,10	69	NE	1	Fair.
26	7 0	58	61	29,98	60	E	1	Fair.
	2 0	62	65	30,03	67	NE	1	Fair.
27	7 0	67	68	30,07	65	E	1	Fine.
	2 0	75	76	30,19	84	SW	1	Fine.
28	7 0	61	58	29,92	60	S	1	Fair.
	2 0	69	72	29,97	75	SW	1	Fine.
29	7 0	63	64	30,00	60	S	1	Fair.
	2 0	77	77	30,07	80	W	1	Fine.
30	7 0	60	59	29,92	57	S	1	Fair.
	2 0	76	76	29,97	81	W	1	Fine.
31	7 0	57	58	29,95	58	SE	1	Fair.
	2 0	70	71	30,02	77	S	1	Fine.

Rain this Month 0,278 Inches.

METEOROLOGICAL JOURNAL.

for September, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Sep.	1	7 0	62	64	29.68	69	NE	1	Cloudy.
		2 0	64	66	29.75	72	NE	1	Slight showers.
	2	7 0	58	59	30.06	60	E	1	Cloudy.
		2 0	65	66	30.02	69	N	1	Fine.
	3	7 0	61	64	29.96	63	NE	1	Cloudy.
		2 0	65	67	29.98	70	E	1	Fine.
	4	7 0	59	61	29.97	60	SE	1	Cloudy.
		2 0	64	63	29.95	68	E	1	Rain.
	5	7 0	61	63	29.97	62	SSE	1	Cloudy.
		2 0	68	67	29.93	66	S	1	Rain.
	6	7 0	64	66	29.75	62	SW	1	Cloudy.
		2 0	60	62	99.87	64	SE	1	Fair.
	7	7 0	57	60	29.91	57	NE	1	Cloudy.
		2 0	61	62	30.07	68	N	1	Cloudy.
	8	7 0	60	62	30.00	61	NW	1	Cloudy.
		2 0	62	64	30.07	68	N	1	Fine.
	9	7 0	62	63	29.96	62	N	1	Cloudy, but fine.
		2 0	69	70	30.06	72	ENE	1	Fair.
	10	7 0	60	60	29.94	62	N	1	Rain.
		2 0	65	67	29.96	70	NE	1	Fine.
	11	7 0	66	66	29.95	61	N	1	Cloudy.
		2 0	67	69	30.07	72	NW	1	Rain.
	12	7 0	62	64	29.96	60	NWbN	1	Cloudy, but fine.
		2 0	66	67	29.94	70	N	1	Cloudy, but fine.
	13	7 0	60	61	30.21	61	NW	1	Cloudy.
		2 0	65	66	29.97	72	WNW	1	Fair.
	14	7 0	68	68	29.93	66	SE	1	Fair.
		2 0	69	70	30.10	77	SE	1	Fine.
	15	7 0	67	68	30.01	65	E	1	Rain.
		2 0	67	69	29.96	70	E	1	Showers.

Rain this Month 0.972 Inches.

METEOROLOGICAL JOURNAL

for September, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Sep. 16	7	0	56	55	29.84	54	NE	1	Fair.
	2	0	69	69	29.77	70	E	1	Rain.
17	7	0	53	54	29.86	56	E	1	Fair.
	2	0	63	63	29.89	65	N	1	Fair.
18	7	0	51	52	30.03	50	SE	1	Fair.
	2	0	63	64	30.05	67	S	1	Rain.
19	7	0	56	55	29.89	55	SSW	1	Fair.
	2	0	63	65	29.93	69	S	1	Rain.
20	7	0	58	60	29.58	56	N	1	Fair.
	2	0	66	66	29.57	68	N	1,2	Rain.
21	7	0	60	61	29.56	60	NE	1	Rain.
	2	0	68	68	29.60	70	NW	1	Rain.
22	7	0	59	60	29.59	61	N	1	Cloudy.
	2	0	63	65	29.68	69	E	1	Cloudy.
23	7	0	59	59	29.68	58	E	1	Cloudy.
	2	0	67	68	29.77	70	E	1	Cloudy but fine
24	7	0	58	59	29.76	57	NE	1	Hazy.
	2	0	66	68	29.81	70	NW	1	Cloudy.
25	7	0	59	60	29.75	56	N	1	Cloudy.
	2	0	65	67	29.55	68	N	1	Cloudy.
26	7	0	57	58	29.57	55	E	1	Fair.
	2	0	68	69	29.66	70	NW	1	Rain.
27	7	0	58	59	29.65	59	N	1	Cloudy.
	2	0	66	66	29.72	68	N	1	Fine.
28	7	0	59	60	29.63	57	SW	1	Fine.
	2	0	63	64	29.77	67	S	1	Fair.
29	7	0	59	61	29.72	59	S	1	Fair.
	2	0	64	66	29.83	69	S	1	Rain.
30	7	0	58	59	29.85	56	SE	1	Fair.
	2	0	63	65	29.96	67	SE	1	Rain.

Rain this Month 0.972 Inches.

METEOROLOGICAL JOURNAL

for October, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Siv's Therm.	Winds.		Weather.	
	H.	M.	o	o	Inches.		Points.	Str.		
Oct.	1	8	0	54	55	29.57	53	E	1	Cloudy.
		2	0	63	64	29.62	68	NE	1	Cloudy.
	2	8	0	51	52	29.43	50	ENE	1	Fair.
		2	0	62	62	29.57	65	NE	1	Rain.
	3	8	0	54	56	29.47	53	E	1	Fair.
		2	0	63	64	29.67	67	E	1	Fair.
	4	8	0	55	56	29.66	55	E	1	Fair.
		2	0	61	62	29.62	65	N	1	Very heavy rain.
	5	8	0	59	59	29.46	57	NW	1	Rain.
		2	0	65	64	29.57	68	N	1	Fair, rain in the night.
	6	8	0	55	55	29.43	53	N	1	Fair.
		2	0	60	61	29.35	65	NW	1	Heavy rain.
	7	8	0	51	53	29.33	50	N	1,2	Cloudy.
		2	0	59	59	29.52	64	N	1	Cloudy. [o'clock, a.m.
	8	8	0	53	53	29.42	51	N	1	Cloudy, very foggy at 9
		2	0	60	60	29.67	63	N	1	Cloudy.
	9	8	0	49	52	29.88	41	NE	1	Cloudy.
		2	0	58	57	29.83	60	W	1	Fine.
	10	8	0	56	58	29.68	53	S	1	Cloudy.
		2	0	58	62	29.53	61	SW	1	Cloudy.
	11	8	0	58	60	29.55	58	S	1	Cloudy.
		2	0	61	61	29.54	63	S	1	Rain.
	12	8	0	51	60	29.72	52	S	1	Fine.
		2	0	57	68	29.83	61	W	1	Fine.
	13	8	0	55	62	29.87	49	SE	1	Cloudy.
		2	0	62	66	29.88	64	S	1	Cloudy.
	14	8	0	57	62	29.99	53	ESE	1	Fine.
		2	0	61	69	29.98	67 $\frac{1}{2}$	S by E	1	Fine.
	15	8	0	59	63	29.95	56	ESE	1	Cloudy.
		2	0	64	66	29.94	67	SSE	1	Cloudy.
	16	8	0	59	64	29.97	57	SE	1	Cloudy.
		2	0	66	66	30.00	66	NW	1	Cloudy.

Rain this Month 1, 166 Inches.

METEOROLOGICAL JOURNAL

for October, 1818.

1818	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H. M.	°	°	Inches.		Points.	Str.	
Oct. 17	8 0	56	64	30.08	56	S	1	Foggy.
	2 0	62	65	30.07	65	ESE	1	Cloudy.
18	8 0	55	62	29.94	51	S	1	Cloudy.
	2 0	61	63	29.95	63	S	1	Fine.
19	8 0	54	62	29.98	54	W	1	Foggy thick weather.
	2 0	58	62	29.98	61	SSW	1	Cloudy and dark.
20	8 0	53	61	30.06	53	SE	1	Cloudy.
	2 0	59	64	30.0	61	SE	1	Fine.
21	8 0	48	59	30.19	44	SE	1	Foggy.
	2 0	54	61	30.15	56	S	1	Fine.
22	8 0	49	57	30.00	48	E	1	Fine.
	2 0	54	57	29.98	55	ESE	1	Cloudy, but fine.
23	8 0	49	56	29.97	54	SE	1	Cloudy.
	2 0	52	55	29.98	54	E	1	Cloudy.
24	8 0	48	55	30.05	48	E	1	Cloudy.
	2 0	50	52	30.15	51½	SE	1	Cloudy.
25	8 0	50	54	30.03	48	E	1	Cloudy and hazy.
	2 0	55	59	30.03	58	E	1	Fine.
26	8 0	51	55	30.06	50	E	1	Hazy.
	2 0	59	60	30.06	60	E	1	Fine.
27	8 0	51	56	30.12	50	E	1	Hazy.
	2 0	58	62	30.14	60	E	1	Fine.
28	8 0	54	57	30.15	52	W	1	Fine.
	2 0	61	61	30.16	61	NW	1	Cloudy.
29	8 0	54	58	30.28	55	W	1	Cloudy thick weather.
	2 0	56	58	30.32	60	W	1	Fine.
30	8 0	51	57	30.30	52	W	1	Fine, rather hazy.
	2 0	57	58	30.07	59	W	1	Cloudy.
31	8 0	53	57	30.07	53	SSW	1	Cloudy.
	2 0	56	58	29.95	58	W	1	Rainy.

Rain this Month 1.166 Inches.

METEOROLOGICAL JOURNAL

for November, 1818.

1818	Time.		Therm.	Therm.	Barom.	Six's	Winds.		Weather.
	H.	M.	without.	within.	Inches.	Therm.	Points.	Str.	
Nov.	1	8 0	51	56	29.90	51 1/2	SW	1	Hazy and cloudy.
		2 0	55	56	29.95	57	W	1	Cloudy.
	2	8 0	50	52	29.94	51 1/2	W	1	Cloudy and hazy.
		2 0	56	57	29.85	59	SW	1	Fine
	3	8 0	53	57	29.77	54	SbyE	1	Fine.
		2 0	55	60	29.71	59	SE	1	Fine.
	4	8 0	51	56	29.54	51	SSE	1	Cloudy.
		2 0	53	57	29.44	57	SSE	1,2	Rain.
	5	8 0	54	57	29.42	54	ESE	1	Foggy.
		2 0	56	61	29.34	59	SE	1	Cloudy.
	6	8 0	54	60	29.34	55	E	1	Cloudy, rain in the night
		2 0	56	64	29.39	59	SE	1	Cloudy.
	7	8 0	52	60	29.65	54	SE	1	Cloudy.
		2 0	55	66	29.76	59	S	1	Fine.
	8	8 0	47	58	29.88	46	S	1	Hazy and foggy.
		2 0	52	57	29.88	57	E	1	Cloudy.
	9	8 0	46	57	29.95	45	E	1	Thick fog.
		2 0	59	59	29.97	58	SE	1	Hazy. [hazy weather]
	10	8 0	48	57	29.97	49	E	1	Small drizzly rain and
		2 0	54	56	29.92	54	SE	1	Cloudy. [night]
	11	8 0	48	56	29.77	49	E	1	Rain, wind and rain in the
		2 0	52	58	29.80	54	E	1	Cloudy.
	12	8 0	43	55	29.68	45	E	1	Fine, rather hazy.
		2 0	49	60	29.62	58	E	1	Fine.
	13	8 0	50	57	29.60	48	E	1	Fine.
		2 0	55	63	29.61	57	E	1	Fine.
	14	8 0	55	59	29.63	53	SE	1,2	Cloudy, rain in the night
		2 0	56	66	29.59	58	S	2	Cloudy.
	15	8 0	50	58	29.53	49	W	1,2	Fine.
		2 0	51	57	29.63	54	W	1	Cloudy.

Rain this Month 1.084 Inches.

METEOROLOGICAL JOURNAL

for November, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Nov. 16	8	0	51	57	29.64	48	W	1	Rain.
	2	0	57	60	29.54	58	NW	1,2	Cloudy.
17	8	0	47	58	29.75	47	W	1	Fine.
	2	0	51	63	29.6	53	W	1	Fine.
18	8	0	42	56	30.04	42	W	1	Fine.
	2	0	52	62	30.07	50	S	1	Fine.
19	8	0	49	57	30.07	46	S	1	Cloudy, and hazy.
	2	0	52	63	30.06	54	SE	1	Fine.
20	8	0	41	58	29.95	42	E	1	Fine.
	2	0	47	59	29.86	48	E	1	Fine.
21	8	0	41	55	29.77	40	SE	2	Cloudy.
	2	0	42	59	29.74	44	E	1	Fine.
22	8	0	39	52	29.75	39	E	1	Cloudy.
	2	0	42	53	29.75	44	E	1	Cloudy, but fine.
23	8	0	49	52	29.55	40	S	1	Rain.
	2	0	54	59	29.61	54	S	1	Fine.
24	8	0	50	57	29.73	49	SW	1	Rain.
	2	0	52	62	29.82	53	WSW	1	Fine.
25	8	0	41	56	30.11	42	W	1	Rain.
	2	0	52	66	30.17	52	S	1	Fine.
26	8	0	51	58	30.14	43	S	1,2	Fine.
	2	0	53	60	30.16	55	SW	1,2	Rain.
27	8	0	52	60	30.48	52	W	1	Cloudy.
	2	0	55	63	30.40	57	W	1	Cloudy.
28	8	0	52	59	30.40	52	SW	1	Fine.
	2	0	56	63	30.44	58	SW	1	Cloudy.
29	8	0	54	59	30.42	54	SW	1	Fine.
	2	0	54	59	30.41	59	SW	1	Cloudy.
30	8	0	51	61	30.23	52	SW	1	Cloudy.
	2	0	53	64	30.21	54	S	1	Cloudy.

K in this Month 1.084 Inches.

METEOROLOGICAL JOURNAL

for December, 1818.

1818	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Dec.	1	8 0	45	61	30,05	49	SE	1	Cloudy.
		2 0	45	62	29,97	50	S	1	Cloudy.
	2	8 0	46	58	30,00	45	NE	1	Cloudy.
		2 0	45	62	30,01	52	W	1	Cloudy.
	3	8 0	47	56	29,65	44	S	2	Rain.
		2 0	48	60	29,61	48	SW	1,2	Cloudy.
	4	8 0	46	57	29,53	46	SE	1	Fine.
		2 0	48	58	29,46	51	SE	1	Cloudy.
	5	8 0	46	55	29,49	46	SSE	1	Fine.
		2 0	50	60	29,55	52	SW	1	Cloudy.
	6	8 0	42	54	29,60	43	SE	1	Fine.
		2 0	49	55	29,61	50	SE	1	Cloudy.
	7	8 0	49	55	29,44	46	S	1	Cloudy.
		2 0	50	61	29,48	53	SW	1	Cloudy.
	8	8 0	50	56	29,72	49	S	1	Rain.
		2 0	50	61	29,87	54	W	1	Cloudy.
	9	8 0	44	56	30,04	44	S	1	Hazy.
		2 0	45	59	30,05	49	S	1	Rain.
	10	8 0	40	53	30,14	39	N	1	Fine.
		2 0	45	58	30,15	46	N	1	Fine, rather hazy.
	11	8 0	38	54	30,12	37	NE	1	Cloudy.
		2 0	43	56	30,16	43	SW	1	Fine.
	12	8 0	36	51	30,14	36	N	1	Cloudy.
		2 0	43	56	30,14	44	NE	1	Cloudy and foggy, dark
	13	8 0	39	52	30,16	38	E	1	Fine.
		2 0	42	52	30,17	43	NE	1	Hazy.
	14	8 0	39	51	30,21	38	N	1	Fine.
		2 0	42	58	30,21	43	N	1	Fine.
	15	8 0	38	52	30,17	38	N	1	Fine.
		2 0	39	53	30,08	39	NNE	1	Foggy.
	16	8 0	32	48	30,02	32	E	1	Fine.
		2 0	34	55	30,08	36	NNE	1	Fine.

Rain this Month 0,722 Inches.

[weather.]

METEOROLOGICAL JOURNAL

for December, 1818.

1818	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.	Winds.		Weather.
	H. M.	°	°	Inches.		Points.	Str.	
Dec. 17	8 0	24	48	30,13	26	W	1	Fine, rather hazy.
	2 0	34	53	30,08	35	NNW	1	Cloudy.
18	8 0	28	46	29,86	28	W	1	Fine, rather hazy.
	2 0	38	54	29,80	40	WbyN	1	Rain.
19	8 0	31	49	30,24	33	W	1	Fine.
	2 0	36	54	30,28	37	W	1	Fine.
20	8 0	43	48	30,13	36	SW	1,2	Cloudy.
	2 0	47	48	30,05	47	NW	1,2	Cloudy.
21	8 0	48	49	30,08	46	W	1	Cloudy.
	2 0	48	49	30,22	48	W	1	Fine.
22	8 0	30	52	30,51	25		1	Thick fog.
	2 0	33	53	30,52	36		1	Thick fog.
23	8 0	32	56	30,43	32	E	1	Thick fog.
	2 0	36	53	30,35	37	E	1	Foggy.
24	8 0	28	48	30,27	30		1	Thick fog.
	2 0	33	53	30,25	35	N	1	Fog.
25	8 0	29	49	30,15	30	W	1	Foggy.
	2 0	37	46	30,08	38	S	1	Hazy and cloudy.
26	8 0	33	45	29,91	35	ESE	1	Cloudy.
	2 0	33	53	29,90	36	SSE	1	Cloudy.
27	8 0	35	44	30,08	35	SW	1	Cloudy.
	2 0	39	45	30,14	40	SE	1	Cloudy.
28	8 0	36	44	30,44	35	N	1	Fine.
	2 0	41	50	30,50	42	N	1	Fine.
29	8 0	35	46	30,61	35	S	1	Cloudy.
	2 0	37	49	30,58	39	N	1	Fine. [white frost.
30	8 0	29	44	30,49	31	NW	1	Cloudy and foggy. A
	2 0	36	51	30,46	37	N	1	Hazy.
31	8 0	33	43	30,43	31	N	1	Thick fog.
	2 0	38	50	30,41	38	N	1	Fine.

Rain this Month 0,806 Inches.

1818.	Thermometer without.			Thermometer within.			Barometer.*			Six's Thermometer.			Rain. †
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	53	28	41,7	59	41	51,4	30,50	29,02	29,82	55	26	41,1	1,461
February	51	26	38,4	62	42	51,6	30,22	29,05	29,72	56	26	37,6	0,727
March	51	33	43,0	68	46	54,5	30,34	28,84	29,67	55	31	43,3	0,986
April	64	40	48,4	68	51	57,9	30,40	29,13	29,71	65	33	48,5	1,791
May	69	48	56,0	68	53	60,4	30,35	29,34	29,84	69	41	55,4	1,597
June	78	57	66,1	77	60	67,8	30,34	29,65	30,03	85	50	66,1	0,407
July	80	61	68,9	81	64	70,4	30,30	29,80	30,05	89	53	69,5	0,361
August	77	53	65,6	77	54	66,4	30,28	29,91	30,08	85	55	68,1	0,278
September	69	51	62,3	70	52	63,5	30,21	29,55	29,86	77	50	64,3	0,972
October	66	48	56,2	69	52	59,6	30,32	29,33	29,87	68	41	56,6	1,166
November	59	39	50,8	66	52	58,7	30,48	29,34	29,85	59	39	51,6	1,084
December	50	24	39,6	62	43	52,7	30,61	29,44	30,07	59	25	40,4	0,806
Whole year			53,1			59,6			29,88			53,5	11,636

* The quicksilver in the basin of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

Mean variation of the magnetic needle, June 1818, $24^{\circ} 15' 43''$ West.

Dip about $70^{\circ} 51'$.

PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

FOR THE YEAR MDCCCXIX.

PART II.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
AND PRINTERS TO THE ROYAL SOCIETY.

MDCCCXIX.

CONTENTS.

- XII. *On the specific gravity, and temperature of Sea Waters, in different parts of the Ocean, and in particular seas; with some account of their saline contents.* By Alexander Marcet, M.D. F. R. S. &c. p. 161
- XIII. *An account of the fossil skeleton of the Proteo-saurus.* By Sir EVERARD HOME, Bart. V. P. R. S. p. 209
- XIV. *Reasons for giving the name Proteo-saurus to the fossil skeleton which has been described.* By Sir EVERARD HOME, Bart. V. P. R. S. 212
- XV. *Some Observations on the peculiarity of the Tides between Fairleigh and the North Foreland; with an explanation of the supposed meeting of the Tides near Dungeness.* By James Anderson, Captain in the Royal Navy. Communicated by the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S. p. 217
- XVI. *On the Ova of the different tribes of Opossum and Ornithorhynchus.* By Sir Everard Home, Bart. V. P. R. S. p. 234
- XVII. *The results of Observations made at the Observatory of Trinity College, Dublin, for determining the Obliquity of the Ecliptic, and the Maximum of the Aberration of Light.* By the Rev. J. Brinkley, D. D. F. R. S. and M. R. I. A. and Andrew's Professor of Astronomy in the University of Dublin. p. 241
- XVIII. *On some new Methods of investigating the Sums of several Classes of infinite Series.* By Charles Babbage, Esq. A. M. F. R. S. p. 249

- XIX. *On the optical and physical properties of Tabasheer.* By David Brewster, LL. D. F. R. S. Lond. and Edin. In a Letter to the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S. &c. &c. &c. p. 283
- XX. *An Account of a Membrane in the Eye, now first described.* By Arthur Jacob, M. D. Member of the Royal College of Surgeons, Ireland; Demonstrator of Anatomy, and Lecturer on the Diseases of the Eye in the University of Dublin. Communicated by James Macartney, M. D. F. R. S. p. 300
- XXI. *A new method of solving numerical equations of all orders, by continuous approximation.* By W. G. Horner, Esq. Communicated by Davies Gilbert, Esq. F. R. S. p. 308

PHILOSOPHICAL TRANSACTIONS.

- XII. *On the specific gravity, and temperature of Sea Waters, in different parts of the Ocean, and in particular seas ; with some account of their saline contents.* By Alexander Marcet, M. D. F. R. S. &c.

Read May 20, 1819.

WHILE analyzing the waters of the Dead Sea and the River Jordan, about twelve years ago, and conversing on the peculiarities of these waters with a late valuable and lamented Member of this Society, Mr. TENNANT, it occurred to us that a chemical examination of different seas, in a variety of latitudes and at different depths, might be interesting; and that, however unlikely to be productive of any striking discovery, such an inquiry, conducted with due care and attention, might afford curious results, and throw some light on this obscure subject. We accordingly began to collect specimens of sea water from various parts of the globe, and it was agreed that I should, aided by Mr. TENNANT's occasional advice, submit them to chemical analysis.

In the course of a few years I became possessed, through

the kindness of several friends, of a great variety of specimens of sea water; and I was preparing to examine them, when a most deplorable accident deprived science of the sagacious philosopher from whose friendship and enlightened assistance I had anticipated so much advantage. Procrastination and delay were the natural consequence of this misfortune; and I should probably have entirely lost sight of the subject, had not my intention been again directed to it by the late expeditions to the Arctic regions, and the great zeal and kindness of some of the officers engaged in them, in procuring for me specimens of sea water, collected in different latitudes, and under peculiar circumstances, so as to add greatly to the value of those which I previously possessed.*

I must not omit to observe, however, that this subject has, at various periods, engaged the attention of philosophers. Thus BERGMAN, † WATSON, ‡ NAIRN, § BLADH, || LAVOISIER, ¶ PAGÉS,** PHIPPS, †† LITCHTEMBERG, ‡‡ PFAFF, §§ BOUILLON-LA GRANGE and VOGEL, |||| &c., have turned their attention to the subject of sea water, and ascertained some valuable, though in general detached and often discordant facts; and more lately the celebrated traveller HUMBOLDT, ¶¶ Dr. MURRAY of

* I am also indebted, both to Sir JOSEPH BANKS and to the British Museum, for various specimens of water from the same expedition.

† BERGMAN's Opusc. Vol. I.

‡ WATSON's Chemical Essays, Vol. V. p. 91.

§ Philos. Trans. for 1776.

|| KIRWAN's Geological Essays, p. 350.

¶ LAVOISIER's Memoirs, 1772.

** PAGÉS' Voyage round the World, from 1767 to 1771.

†† PHIPPS' Voyage towards the North Pole, 1773.

‡‡ LITCHTEMBERG, in SCHWIGGER's Journal II. 256.

§§ PFAFF, *ibid.*

|||| Annales de Chimie, Vol. LXXXVII. ; and Ann. of Philos. IV. 200.

¶¶ HUMBOLDT, Relat. Historique, Vol. I.

Edinburgh,* Dr. JOHN DAVY, now of Ceylon,† and M. GAY-LUSSAC,‡ have also added many useful facts to this part of natural science. These two last observers, in particular, have given the specific gravities of waters in a variety of latitudes, from England to Ceylon, and from South America to France, and their results appear to lead to the general conclusion, that the variations obtained in those parts of the ocean were but very slight, and are to be ascribed rather to accidental causes than to any general principle.§

The immediate objects of investigation which presented themselves to me were, first, to ascertain the specific gravities of upwards of seventy specimens of sea water which I had procured from different parts of the world; and afterwards to examine whether any differences could be traced in the chemical composition of those waters. This naturally gave rise to two distinct parts, and afforded a convenient mode of dividing the subject.

§ 1. *Of the specific gravities of sea waters, from different seas, and in different latitudes; with some remarks on the temperature of those seas.*

Before I proceed to state the results, I shall briefly describe the mode in which the waters were weighed, and the apparatus which I contrived for the purpose of raising water from the bottom of the sea.

The specific gravities were taken in the usual mode, that is, by comparison with equal bulks of distilled water of the same

* Edinb. Philos. Trans. Vol. VIII.

† Philos. Trans. for 1817.

‡ Ann. de Chim. for Dec. 1817; and Philos. Magaz. Vol. LI.

§ M. De HUMBOLDT (*Personal Narrative*, Vol. I.) thought he could perceive that

temperature.* The balance I used was one which was sensibly affected by $\frac{1}{50}$ th part of a grain; but I did not think it necessary to use smaller weights than $\frac{1}{20}$ th part of a grain: so that whenever, in the annexed tables, smaller weights are expressed, in the sixth decimal figure, such very minute parts must not be understood to have been obtained from actual experiment, since they resulted, by calculation, from the conversion of the weights actually obtained into the usual standard of 1000 parts.

The first idea of the apparatus which I contrived for raising water from the bottom of the sea, occurred to me about ten years ago, on accidentally seeing in an instrument maker's shop, a machine, said to be the identical one which was used for a similar purpose by Dr. IRVING, in Captain PHIPPS's (since Lord MULGRAVE) expedition.† This consisted simply in a cylindrical vessel having an opening at the top, and a similar one at the bottom, each closed by a flap or valve opening only upwards, and moving freely upon hinges. When this sea water was less salt between the tropics than between the coasts of Spain and Teneriffe; and his observations seem to lead to the inference, that there is a specific gravity peculiar to the water of each zone; a conjecture, however, which the facts collected in this paper do not appear to confirm.

* It may perhaps be worth while to mention a small improvement which was introduced in the vessel used for weighing the waters. The apparatus consisted in a thin phial, nearly spherical, containing between five and six hundred grains of distilled water, and having a very light ground glass stopper. But as I had observed on former occasions that such phials were apt to burst on the stopper being forced in, from the compression of the liquid with which they were filled, I had the stopper made with a very small longitudinal aperture through it, so as to allow a minute quantity of water to ooze out; and this was very easily accomplished (by Mr. NEWMAN, of Lisle-street) by forming the stopper of a portion of thermometer tube, the bore of which perfectly answered the desired purpose.

† PHIPPS's Voyage to the North Pole in 1773.

apparatus was sunk into the sea, the valves would, of course, be kept open by the current of water passing freely through the machine so long as it descended; and when drawn up again, the valves would be kept closed by the water acting in an opposite direction. As however, in heaving a machine of this kind from a great depth, it is almost impossible that some oscillations should not take place in it, either from the motion of the boat, or from some accidental jerks in pulling the line, it is evident that these oscillations being necessarily communicated to the valves, partial changes of the water contained in the vessel are almost unavoidable. It was principally with a view to remove this objection, that I modified the principle of the apparatus in the following manner. I procured a strong cylindrical brass vessel, capable of holding about half a pint of water, and having, like Dr. IRVING's machine, an aperture both at the top and bottom, with a valve at each aperture opening upwards. But these valves, instead of being allowed to move backwards and forwards freely upon their hinges, were, when closed, firmly locked by springs; and when open, could only be kept in that state by the application of a certain degree of force. This force consisted in a weight of several pounds, suspended to the extremity of a cord, the other end of which was fastened to the valves; and the machine was so constructed (as will be readily understood by referring to the annexed sketch, Plate XI.), that the moment the valves were relieved of the weight which kept them open, they closed instantly, and were forcibly locked by the above mentioned contrivance. Now the machine being open, with the weight suspended to it at a few feet distance, and the whole being let down into the sea, it is evident that the apparatus must continue perfectly open

and pervious till the weight touches the bottom, at which moment it closes itself, and remains, in that state. The water brought up in this machine, therefore, can only come from the bottom, and from no intermediate depth; for whenever the apparatus fails in reaching the bottom, it continues pervious, and on being raised out of the water is found open, and emptied of its contents. The advantage however of raising water from the bottom with certainty, whenever the bottom can be reached, is a valuable one, and has already enabled me to ascertain some curious points respecting the sea of Marmora, as will be presently seen; and if some mode could be contrived of closing the machine at any desired intermediate distance from the surface (an object to which I have directed the attention of the ingenious instrument-maker, Mr. NEWMAN), it it would then answer every desired purpose.*

* Since this paper was written, Mr. NEWMAN has succeeded in constructing a machine, (just in time to be put on board of Lieut. PARRY's ship, about to sail to Baffin's Bay), which seems well calculated to answer the desired object, under any circumstances. The principle is essentially the same as that of the machine which I have just described, namely, that of closing itself when it touches the bottom; but with this material addition, that the valves, when the bottom cannot be reached, may be closed at any given depth, by causing a weight to descend along the cord to which the machine is suspended, till it comes into contact with it, and closes the valves by an appropriate contrivance. A sketch of this machine, with its explanation, is also annexed at the end of this paper, Plate XII.

Among the attempts which have been made to bring up water from any given depth, I should not omit to mention an ingenious contrivance of Mr. TENNANT, which he thought calculated to answer the purpose tolerably well, and which, as will be seen hereafter, was used some years ago with apparent success.

Mr. TENNANT's contrivance consisted in a wooden box, capable of holding a few ounces of water, and opening like a snuff-box, by a lid moving upon hinges, and fitting water-tight. The box, when closed, was forcibly kept in that state by a spring; but when about to be used, it was partially opened, and was prevented from shutting itself

I shall now proceed to state the results which I have obtained in regard to the specific gravities of sea water ; and for the sake of conciseness I shall present them in the form of

again by means of a small spring or wedge. It remained open so long as it descended ; but when pulled up again, the wedge was thrown out of its place, and this obstacle being removed, the box closed itself instantly. This was effected by means of a small fly-wheel, so confined as to admit of no motion during the descent of the machine ; but the moment it began to ascend, the pressure of the water communicated to the wheel a rotatory motion, which, through a little mechanical contrivance, disengaged the spring and closed the box.

Another, and more elaborate invention, of the same kind, (a description of which may be found in the ‘ Journal of Science and the Arts, Vol. V.) was devised last year by Sir HUMPHRY DAVY, and was repeatedly used in the late voyages towards the North Pole ; and in fact, many of the specimens of waters from those regions, which I have examined, were raised by means of that ingenious, though I apprehend, not unexceptionable apparatus.

The principle of this instrument may be stated in a few words. It consists in a strong copper bottle of an oblong shape, closed at its neck by a stop-cock. To this bottle is attached laterally, and in a parallel direction, a metallic tube closed at the top and open at the bottom, with an air-tight piston moving within the tube. As the open end of the tube therefore descends into the sea along with the bottle, the piston which closes the orifice of the tube is gradually forced upwards into it, as the machine sinks, the air within it being proportionally compressed ; but when the piston has reached a certain part of the tube, it meets with a catch and opens the cock of the bottle, which, of course, instantly fills with water ; and there is an ingenious contrivance by which the machine may be set before hand, so as not to let in the water till a certain known degree of pressure is made by the superincumbent column.

Captain Ross, in his account of his ‘ Voyage to the Arctic regions’ (Appendix, page cxxiv.), makes the following observation : ‘ Sir HUMPHRY DAVY’s apparatus answered the purpose for which it was intended ; but it did not close so as to prevent the water from escaping or mixing with that nearer the surface as it came up.’ This objection however, which might probably be removed, is of no great importance, as the aperture in question being very small, and situated at the upper end of the vessel, there is no risk of any sensible change taking place in the contents of the vessel through that aperture, while traversing the upper strata. But the machine appears liable to the

tables, taking care to annex to each specific gravity, under the head ‘ observations,’ any collateral information which I may be able to offer.

more serious objection of depending for accuracy upon very nice adjustments, which can hardly be relied upon under very great pressures.

Lieut. FRANKLIN, who commanded his Majesty’s ship the *Trent*, and was not provided with one of Sir HUMPHRY DAVY’s machines, sometimes used a cylindrical leaden vessel with two valves, like that employed by Dr. IRVING; and at other times he made use of an empty corked bottle, the cork being strongly tied to the bottle. In every instance the bottle was found filled, the cork having been forced into the bottle; except on one occasion, on which the bottle had filled itself without the cork being driven in, not apparently from its being more strongly fastened than in other cases, but rather probably from its being more porous, so as to allow the water to filter through it before the pressure was sufficient to move it from its position. Lieut. FRANKLIN had with him my instrument; but he used it only in a few instances, on account of its being too light to reach great depths, and of too delicate a structure to render the addition of a very large weight practicable; an objection however which may very easily be remedied. From all these circumstances it is easy to perceive, that the means used for raising water from great depths, have hitherto been far from uniform in their principle, or certain in their performance.

TABLE I. *Specific Gravities of Sea Waters.*

Designation of Seas.	Nos. of Specimens.	Latitude.	Longitude.	Specif. Grav.	OBSERVATIONS.
Arctic Ocean.	1	66,50	68,30W	1025,55	Taken up by Captain Ross, in Sept. 1818, from a depth of 80 fathoms, with Sir Humphry Davy's apparatus. Temperature of the water at 80 fms. 30°; at 200 fms. 29°; at 400 fms. 25,5; at 670 fms. 25°; at the surface 83°. Temperature of the air, 31°. Bottle labelled in Capt. Ross's own hand-writing, with all the above particulars.
	2	74,0	—	1025,46*	By Lieut. Parry, from the surface. The ship surrounded by ice in every direction. Temperature of the water 31°, of the air 34°; 8 July, 1818.
	3	74,50	59,30	1026,19	By Lieut. Parry. Temperature of water 32°, of air 36°.
	4	75,14	4,49E	1027,27	By Lieut. Franklin, from the surface, 10 Sept. 1818.
	5	75,14	4,49	1027,27	By Lieut. Franklin, raised with the cylindrical machine, from a depth of ~36 fms. Temperature of the water brought up 36°, of the air 35°; 10 September, 1818.
	6	75,54	65,32W	1022,7*	By Captain Ross, from the surface, 4 miles from the land; 12 August, 1818.
	7	75,54	65,32	1025,9	By Capt. Ross; from a depth of 80 fms. with Sir H. Davy's machine. Soundings 150 fms. 12 August, 1818.
	8	76,32	76,46	1024,05*	By Capt. Ross; from the surface. Soundings 109 fms. 22 August, 1818.
	9	76,32	76,46	1026,22	By Capt. Ross; from a depth of 80 fms. Temperature 30,5°; 22 August, 1818.
	10	76,33	—	1026,64	By Lieut. Parry; with Sir H. Davy's machine, from a depth of 80 fms. Temperature of the water 32°, of air 36°; 21 Aug. 1818.
	11	79,57	11,15E	1026,7	By Lieut. Franklin, from a depth of 34 fms. Temperature of the sea at the surface 30,3°, at 34 fms. 33,2°; of the air 35,2°.
	12	80,26	10,30	1022,55*	By Lieut. Franklin, 13 July, from the surface; ship beset with ice; 12 leagues from the Coast of Spitzberg. Temperature of the surface, 32,5°, of air 35°.
	13	80,26	10,30	1027,14	By Lieut. Franklin; from the bottom, depth of 237 fms.
	14	80,26	10,30	1027,15	By Lieut. Franklin: from the bottom, depth of 237 fms. with Dr. Marcet's machine. Temperature of the bottom 35,5°; 18 July, 1818.
	15	80,28	10,20	1026,8	By Lieut. Franklin; from bottom, depth of 185 fms. surface being frozen. Temperature of the bottom 36,4°, surface 32,7°; 15 July, 1818.
	16	80,29	11,0	1026,84	By Lieut. Franklin; from a depth of 305 fms. being the bottom. Temperature of the air 36°, of the surface of the sea 32,2°; 18 July, 1818.

N. B. The specimens marked * in the three first tables, cannot be taken into account in calculating the mean specific gravity of the waters of the ocean, their saline contents being much diminished either by the vicinity of large masses of ice, or of great rivers, which reduce them much below the average standard of density of sea water.

TABLE II. *Specific Gravities of Sea Waters.*

Designation of Seas.	Nos. of Specimens.	Latitude.	Longitude.	Specific Gravity.	OBSERVATIONS.
Northern Hemisphere.	17	63,49	55,38W	1026,7	By Lieut. Parry, in July, 1818; from a depth of 80 fms. Temperature of the water $33\frac{1}{2}^{\circ}$ of the surface 33° , of the air $32\frac{1}{2}^{\circ}$
	18	59,40	14,46	1030,04	By Capt. Basil Hall; from the surface. In July, 1811.
	19	56,22	—	1026,56	Taken up by Dr. Berger, about 15 leagues from the West Coast of Jutland; depth 23 fms. Dec. 1810.
	20	54,0	4,30	1026,8	By Dr. Berger; Calé of Man, Irish Sea.
	21	53,45	0,20	1026,7	By Dr. Berger; near Hull.
	22	52,45	4,0	1021,75*	By myself; from Barmouth, Wales, near the mouth of the river Mawdack.
	23	48,25	6,34	1030,02	From Mr. Tennant; taken up by Mr. Lushington.
	24	46,0	48,0	1026,48	By Mr. Caldwell, Coast of Canada. Temperature of water 42° , of air 50° .
	25	45,20	45,10	1028,16	By Mr. Caldwell; brought up from a depth of 250 fms. by means of a corked bottle.
	26	45,10	15,0	1029,34	By Capt. Hall, in January, 1811.
	27	25,30	32,30	1028,86	By Capt. Hall; nearly in the middle of the North Atlantic.
	28	22,0	89,0E	1020,28*	By Capt. Hall; from the mouth of the Ganges, about 20 miles from Calcutta. Water muddy.
	29	13,0	74,0	1027,72	By Capt. Hall; Coast of Malabar, off Cochin; some sediment, apparently vegetable.
	30	10,50	24,26W	1028,25	By Mr. Schmidtmeier, going to South America; bottle blackened, smell hepatic.
	31	7,0	80,E	1030,9	From Mr. Tennant, by Mr. Lushington, off Columbo, Ceylon.
	32	4,0	23,W	1027,72	By Mr. Schmidtmeier, in April, 1808. Therm. 84° .
	33	3,28	81,4E	1030,22	From Mr. Tennant, by Mr. Lushington.
	34	0,	25,30W	1028,25	By Mr. Schmidtmeier.
	35	0,	23,0W	1027,85	By Capt. Hall, in August, 1817.
	36	0,	83,0E	1028,07	By Capt. Hall, in 1815; about 300 miles south of Ceylon.
Equator.	37	0,	92,0E	1026,92	By Capt. Hall; 8 or 400 miles west of Sumatra, June, 1817.

The specific gravity of the specimens marked * in this and the following Table, being obviously much less than common, in consequence of the vicinity of rivers, these specimens have not been taken into account in calculating the mean specific gravity of sea-water.

TABLE III. *Specific Gravities of Sea Waters.*

Designation of Seas.	Nos. of Specimens.	Latitude.	Longitude.	Specific Gravity.	OBSERVATIONS.
Southern Hemisphere.	38	8,30 S.	32,0 W.	1028,95	Taken up by Mr. Schmidtmeier, in May 1808. Temperature 82°.
	39	9,0	35,0	1029,20	By Mr. Schmidtmeier, at Pernambuco.
	40	11,30	33,7	1029,80	From Mr. Tennant, by Mr. Lushington.
	41	21,0	0,	1028,19	By Capt. Hall, near the middle of the South Atlantic.
	42	23,30	73,0 E.	1028,31	By Capt. Hall, Tropic of Capricorn, between Madagascar and New Holland.
	43	25,30	5,30	1032,09	By Capt. Hall; about half way between St. Helena and the Cape; in June, 1815.
	44	28,0	43,0	1027,15	By Capt. Hall; Mosambique, South of Madagascar.
	45	35,0†	56,0 W.	1025,45	From Mr. Tennant, by Mr. Lushington; mouth of the Rio de la Plata.
	46	35,10	21,0 E.	1027,5	By Capt. Hall; South of the Cape, on the Banks of Lagullas.
	47	35,33	0,21	1031,6	From Mr. Tennant, by Mr. Lushington; phial partly emptied.
Yellow Sea.	48	35,0 N.†	—	1022,91	By Captain Hall, in 1816. There were several phials of this water, with glass stoppers. All the phials were blackened internally by the water, which had a highly hepatic smell. This water, when seen in large masses, has a greenish yellow colour.
	49	36,0 N.†	5,0 W.	1030,1	By Dr. Macmichael, in 1811, from a depth of 250 fms. in the Straits of Gibraltar, between Cape Europa and Cabrita, with Mr. Tennant's machine.
	50	36,0 N.†	5,0	1030,5	By Dr. Macmichael; from the same spot as the preceding, but from the surface.
	51	—	—	1027,3	By Mr. Tennant; taken up by himself at Marseilles, in 1815; Latitude not specified.
Mediterranean.					

† The Latitudes thus marked are stated only as approximations, not being specified on the labels of the bottles.

TABLE IV. *Specific Gravities of Sea Waters.*

Designation of Seas.	Nos. of Specimens.	Latitude.	Longitude.	Specific Gravity.	OBSERVATIONS.
Sea of Marmora.	52	40,5 N	26,12 E	1028,19	Taken up by Sir Robert Liston, at the entrance of the Hellespont or Dardanelles, <i>from the bottom</i> , 84 fms. deep, by my machine, in June 1812.
	53	40,5	26,12	1020,28	By the same Gentleman, and exactly from the same spot as the preceding, but <i>from the surface</i> .
	54	41,0†	29,0	1014,44	By Sir Robert Liston; at the entrance of the Bosphorus or North entrance of the channel of Constantinople, about four miles from the land; <i>from the bottom</i> , 80 fms.
	55	41,0†	29,0	1013,28	By the same Gentleman; same spot, but <i>from the surface</i> .
Black Sea.	56	—	—	1014,22	} By Mr. Sautter; one of the specimens clear; the other slightly hepatic. Latitudes not stated.
	57	—	—	1014,14	
White Sea.	58	65,15 N	39,19 E	1018,94	By Mr. Sautter, in 1811. Water perfectly clear.
	59	—	—	1019,09	By the same; latitude not noted.
Baltic.	60	56,0 N	15,0 E	1004,9	By Mr. Prevost, in Carlsham harbour; cork and bottle slightly blackened.
	61	57,39	—	1025,93	By Dr. Berger; in 1810, Categat, one mile and a half from the Eastern coast of Jutland. Depth about 14 fms.
Ice-Sea Waters.	62	56,0	12,40 E	1015,87	By Dr. Berger; from the Sound, or Passage into the Baltic, halfway between Denmark and Sweden. Depth about 17 fms.
	63	75,54 N	65,32 W	1000,	By Captain Ross; from the same spot as No. 6 and 7; sounding 150 fms. from an Iceberg. 12th August, 1818.
	64	80,28	10,20 E	1000,17	By Lieutenant Franklin, from <i>water at the surface</i> , when beset amongst ice. Same spot as No. 15. Temper. of the surface 32, 5°, — 15 July, 1808.
	65	79,56	11,30	1000,6	By Lieutenant Franklin, from a floe, the ice being 14 feet deep under the surface; 21st. of June, 1818.
	66	79,38	11,0	1000,15	By Lieutenant Franklin, from an immense iceberg. August, 1818.
	67	76,48	13,40	1002,35	By Lieutenant Franklin, on the 26th May, 1818. About 90 miles from Spitzberg, Temp. of air 29°. Soundings 600 fms. Taken from the surface of a small detached piece of ice floating in the sea.
	68	75,40	61,20 W	1000,15	By Lieutenant Parry, from young ice on the surface, about $\frac{1}{2}$ inch thick, July 31, 1818.

† The Latitudes and Longitudes thus marked were inferred from the description of the spot, not being stated on the bottles.

In endeavouring to connect together the various statements contained in the above tables, the following inferences present themselves.

The ocean in the southern hemisphere, would appear to contain more salt than in the northern hemisphere, in the proportion of 1029,19 to 1027,57 ; as may be seen by taking the mean specific gravity of the waters collected from the two hemispheres. But it must be observed, that a great proportion of the specimens from the northern hemisphere were taken farther from the equator than those procured from the other hemisphere, which may possibly account for the difference in question.*

The mean specific gravity of specimens taken from various parts of the equator is 1027,77, and is therefore a little greater than that which prevails in the northern hemisphere, though sensibly less than that of the southern ocean.

There is no notable difference between different east and west longitudes at the equator ; nor is there, in other latitudes, any material and constant difference between waters of the ocean in corresponding east or west longitudes in the same hemisphere.

There is no satisfactory evidence of the sea, at great depths, being more strongly impregnated with salt than it is near the surface ; except under peculiar circumstances, which will

* It may be observed also that Dr. DAVY, in the experiments abovementioned (Philos. Trans. 1817), generally found the specific gravity of sea water, both in the South Atlantic and in the Indian Ocean, lower than I have done ; for which I am at a loss to assign any reason, unless it be supposed that some of my specimens, from having been long kept, and perhaps not corked with sufficient care, may have undergone some degree of concentration.

hereafter be explained, and appear independent of any general law.

In general the waters of the ocean, whether taken from the bottom or from the surface, appear to contain most salt in places in which the sea is deepest or most remote from land ; and the vicinity of large masses of ice seems to have a similar effect to that of land in diminishing the saltiness of the sea. If, therefore, in attempting to approach the Pole, the saltiness of the sea should appear to increase, and become more uniform at the surface, such a circumstance might be considered as militating against the probability of the sea being extensively frozen in those regions.

It may be stated generally, that small inland seas, though communicating with the ocean, are much less salt than the open ocean. This is particularly striking in the case of the Baltic ; and also, though in a less remarkable degree, in the Black sea, in the White sea, in the sea of Marmora, and even in the Yellow sea.*

The Mediterranean, though a comparatively small and subordinate sea, is found to contain rather a larger proportion of salt than the ocean.† This appears to form an exception

* The Caspian sea is also said, but upon no certain authority, to be less salt than the ocean. Its waters having, like those of the Dead sea, no obvious communication with those of any other seas, present a particular case well deserving of investigation ; and I regret that I have not yet been able to procure a specimen of them, notwithstanding the various attempts which I have made for that purpose.

† This has been stated by various writers, and appears to be the case from the few specimens which I have examined ; but I cannot speak with perfect confidence on this point, as I was but scantily supplied with water from that sea, though comparatively so near and so much frequented by navigators of all descriptions. In their analysis of sea water, BOUILLON-LA GRANGE, and VOGEL state the proportion of saline matter in the water of the Mediterranean to be 41, that of the Atlantic being 38, and that of the English Channel 36.

to the general fact which I have stated above, and stands in need of explanation.

In order to account for this, it has been argued that the Mediterranean sea is not supplied by the rivers which flow into it with a quantity of fresh water, sufficient to replace that which it loses by evaporation under a burning sun, assisted by a powerful radiation from the African shores, and the parching winds blowing from the adjacent deserts; so that a current from the ocean is required to replenish this waste, and prevent the Mediterranean from sinking below its level. Accordingly it is observed that a current sets in at all times from the ocean into the Mediterranean, which current, I am informed, is so strong at Gibraltar, as to carry a ship at the rate of two or three miles an hour; and it is felt as far eastward as the Cap de Gat, a distance of upwards of one hundred and fifty miles; so that ships going out of the Mediterranean, scarcely ever attempt to beat out against contrary winds, and usually keep close either to the African or European shore, in order to avoid the full force of the stream.

If this hypothesis, however, of a disproportion between the loss of water produced by evaporation, and the inadequate compensation afforded by the ingress of rivers, be founded in fact; and if this deficiency be replenished by the saline waters of the ocean, it will be necessary to explain why the waters of the Mediterranean do not gradually increase in saltness, and indeed how it happens that they are not ultimately converted into a saturated brine. It has been supposed, in order to remove this difficulty, that an under-current of water, saltier than the ocean, runs out of the Mediterranean at the Straits

of Gibraltar, and unloads its waters of their excess of salt. But however plausible this theory may be, it must be confessed, that scarcely any other argument has hitherto been alledged in support of the probability of this under-current, than the easy explanation it would afford of the phenomena, and analogies derived from the familiar fact of opposite atmospheric currents formed in confined places, from the mere admission of air of a different temperature.* The following fact, however, for which I am indebted to Dr. MACMICHAEL, who had it from very respectable authority (the British Consul at Valentia), seems to give considerable support to the above theory. Some years ago a vessel was lost at Ceuta, on the Coast of Africa, and its wreck afterwards thrown up at Tariffa, on the European shore, full two miles west of Ceuta. How can this be explained, except by the action of what may be called a counter-submarine current, which would carry a body, sunk to a considerable depth, out of the Straits?

It was a favourite scheme of the late Mr. TENNANT, to examine specimens of sea water from the Straits of Gibraltar, taken both from the surface and from some great depth, in order to ascertain whether the latter would have a greater specific density than the former, a circumstance which, if it did not establish the truth of the theory in question, would at least render it very probable. It was with a view to decide this point, that Mr. TENNANT constructed the machine which

* Thus it is well known that if the door of a heated apartment be partially opened, and two lighted candles placed the one at the top and the other at the lower part of the aperture, the uppermost candle will have its flame propelled outwards, by the rushing out of the heated and therefore lighter current, while the other candle will have its flame blown inwards by the opposite effect.

I have before mentioned, by means of which he flattered himself that water could be brought up from any desired depth ; and it was upon the same occasion that I contrived the apparatus above described, in hopes that it would enable me to obtain water from the bottom of the Straits. My friend, Dr. MACMICHAEL, one of the travelling Fellows of the University of Oxford, and Member of this Society, undertook to make the attempt. He succeeded in procuring water in the Straits, from the depth of two hundred and fifty fathoms, with Mr. TENNANT'S machine ; but all attempts to obtain water from the bottom proved fruitless, from the impossibility of reaching it on account of the very great depth of the sea in that spot.* The specimens of water, however, procured by Mr. TENNANT'S machine, were sent home, and were soon afterwards examined, in the presence of Dr. MACMICHAEL, by Mr. TENNANT, who could not detect any difference in their specific gravity ; and when I lately re-examined the same specimens, which had been preserved, it even appeared (probably from some accidental circumstance) that the specimen from the surface was a little heavier than the other. This point therefore remains to be decided by farther investigation.

With regard to the waters of the Atlantic, although no pains have been spared by the able and zealous officers employed in the late voyages towards the Pole, to procure specimens of water, both from the surface and from great depths, with a view to compare their densities, and though I have

* This attempt was made in Sept. 1811, in the Bay of Gibraltar, between Cape Europa and Cabrita. See Table III.

been favoured with many of those specimens, I have not been able to obtain results sufficiently conclusive to enable me to form a decided opinion upon the subject. On referring to the annexed tables, it will be observed, that, in a variety of instances, the water at the surface was much lighter than when procured from some depth; but then it would appear that whenever such a result was obtained, it was owing to the water at the surface being diluted by the melting of large masses of ice; for under ordinary circumstances (as in the case marked No. 5, Table I.), no such difference was obtained between waters taken at the surface, or brought up from a considerable depth; and in no instance did the density of the water of the Atlantic, from whatever depth it was obtained, appear to exceed the mean density of the waters of the ocean.

The fact however may be, and actually appears to be different in the case of particular seas or arms of the ocean, in which the influence of currents and other local circumstances is more sensibly felt, and the waters of which do not, for obvious reasons, necessarily partake of the uniform saltiness of the ocean. The experiment, as was before observed, does not appear to have yet been fairly tried in the Mediterranean, and indeed from the great depth of that sea, it must be extremely difficult, if not impracticable, to raise water from the bottom, at least at any considerable distance from the coast.

In the instance of the sea of Marmora, in which water was obtained with certainty from the bottom, by means of my machine* (Table IV. Specim. 52 to 55), the result was very

* It was through the kindness of the British Ambassador at Constantinople, Sir ROBERT LISTON, that these specimens were procured. Sir Robert has since told me that the use of the machine was not attended with the least difficulty.

remarkable. The difference of specific gravity between the upper and lower strata, at the entrance of the Dardanelles, where the depth is very moderate, proved to be nearly as 1020 is to 1028; a very curious result, which gives additional plausibility to the hypothesis just mentioned respecting the entrance of the Mediterranean.

Among the specimens of sea water collected by Mr. TENNANT, a small phial of water was found, which had been sent him from Persia, by his friend, the unfortunate traveller BROWNE, a short time before he was murdered.* This interesting specimen was taken from the Lake *Ourmia* or *Urumea*,† a small sea situated in the province of Azerbijan in Persia, south-west of Tabreez, and at no great distance from the Volcanic region of Mount Ararat. This lake is thus described by KINNEIR, in his "Geographical Memoirs of the Persian Empire," page 155: "The Lake Urumea, generally believed to be the Spanto of Strabo, and Marcianus of Ptolemy, is 80 fursungs, or according to my computation about 300 miles in circumference. The water is more salt than that of the sea, no fish can live in it, and it emits a disagreeable sulphureous smell. The surface is not however, as has been stated, incrustated with salt; at least it was not so in the month of July, when I saw it; on the contrary, the water was as pellucid as that of the clearest rivulet." Such salt lakes, entirely unconnected with the ocean, being by no means of frequent occurrence, I propose to give, in another part of this paper,

* I was indebted for this water to my friend Mr. WARBURTON, who put it into my hands after Mr. TENNANT's death.

† Called also Lake of Shahee (See MORIER's Second Journey to Persia, page 286).

some account of the saline matter contained in the waters of Lake Ourmia. I shall only state at present that the specific gravity of the specimen of water in my possession, which appears to have been very carefully preserved, is no less than 1165.07, a degree of saline impregnation which has not, I believe, been observed in any other lake, with the exception of the Dead sea, the waters of which are even heavier.

The excellent opportunities which occurred, during the late northern expeditions, for procuring specimens of water from the various kinds of ice which are met with in those regions, and the obliging zeal of its commanders, afforded me the means of making some inquiries into the nature of these waters. With regard to the floating masses of ice called *icebergs*, which are formed from the waters of melted snow, and are detached by rain and torrents, or by their own weight, from the vallies and from precipitous rocks along the shores, it was long known that they consist of fresh water, in a state of great purity, though perhaps seldom so perfectly pure as the specimen marked 63 (Table IV.), the specific gravity of which was exactly 1000. But the immense fields of ice, or *floes*, which are formed from the actual congelation of the surface of the sea, are of a different description. This ice, generally speaking, is not so compact or so transparent as the icebergs, and it is even stated, in a late curious and elaborate dissertation on the subject of the polar seas, published in the *Edinburgh Review*,* that this ice is

* *Edinburgh Review*, vol. XXX. page 15. There is also in the 4th volume of the 'Journal of Science and the Arts,' a paper of Mr. SEEVER, which was read in 1815, before the Wernerian Society, and contains many curious and valuable observations.

“porous, and consists of spicular shoots or thin flakes, which detain within their interstices the stronger brine; that it can therefore never yield pure water; though if the strong brine be first suffered to drain off slowly, the loose mass which remains will melt into a brackish liquid,” &c. This statement however seems to have been founded rather upon results obtained from the artificial congelation of sea water, than from the examination of the sea ice itself; for it will be seen, by a reference to Table IV., that this ice yielded, in every instance, water considerably purer than we commonly find spring water, or even river water.* Thus, for instance, water from young ice scarcely exceeding half an inch in thickness (Table IV. spec. 58), was found to have a specific gravity of only 1000,15; and yet Lieutenant PARRY, by whom this specimen was taken up and brought home, told me that he had not used the precaution of wiping the ice before he suffered it to melt, a circumstance which is more than sufficient to account for the minute quantity of saline matter which it contained.

It appears therefore well established that sea water, when in the act of passing to the state of ice, parts with the whole, or nearly the whole of its salt to the lower and denser strata; and it may be inferred also from several of the results men-

* I found the specific gravity of the water of the Thames, taken from a large cistern in Lombard-street, 1000,43. The water was quite clear; and, from the cistern being filled at different periods of the tide, afforded a good average of that water at London Bridge. I found the specific gravity of the water of the New River, taken from a cistern in my own house, 1000,52; and I was rather surprised to find that the specific gravity of a specimen of spring-water, from a well in Russell square, was only 1000,17.

tioned in the annexed tables, that this separation of saline matter does not exclusively belong to ice actually formed; but that it also prevails, more or less, in water which is only approaching to the state of ice, or has just passed to the liquid state; so that (as appears from specimen 64), there are circumstances under which water may be found on the surface of the sea almost entirely deprived of its saline contents; which fully accounts for the great difference of density observed in the northern ocean, between the surface of the sea and its lower strata. This separation of the saline matter had long been shown, by experiments upon a small scale, to take place during the freezing of sea water; and Mr. NAIRNE, who ascertained this point so far back as the year 1776,* states that this congelation takes place at the temperature of about $28,5^{\circ}$, the water thus frozen being almost entirely freed from its salt. Upon trying a similar experiment with the air pump, in the mode invented by Mr. LESLIE for artificial congelation, I found that I could, without the least difficulty, and in the course of a few minutes, freeze sea water of the specific gravity of 1026. The water congealed when the thermometer reached 27° ; then it rose to 28° , and remained at that temperature. This experiment being repeated with another portion of the same water, but more slowly and with weaker sulphuric acid, the temperature gradually sunk to 20° , when the whole mass froze at once, the ice being quite smooth and not at all frothy, though it did not exhibit the dry snowy surface which is observed in the freezing of fresh water under similar circumstance. The thermometer, as in the former case, rose to 28° instantly, and

* Philosop. Trans. Vol. 66.

remained stationary at that point. The ice being taken out of the receiver and the vessel inverted, a small quantity of strong brine drained off from it. This was mixed with the portion of water, which, in the former experiment, had escaped congelation, and the specific gravity of these mixed unfrozen residues proved to be 1035,16; whilst that of the frozen portions, after being washed with distilled water, then melted and mixed together, was 1015,2. These results seem to show that a certain degree of rest and slowness in the process, and probably also a certain mass of water, are conditions required for the entire separation of the salt; and hence we are enabled to account for the slight differences which we observe in this respect, in various specimens of water taken from the frozen surface of the sea.*

With regard to the important questions connected with the temperature of the Arctic seas, it will be seen by a reference to the tables, that this interesting subject of inquiry was not overlooked in the late Northern Expeditions, and that some curious observations were made in those regions, on the temperature of the sea, both at the surface and at different depths. Thus for instance it is stated in the 1st. Article of Table I., on the authority of Captain Ross, that in lat. 66,50, and long. 68,30, west, the temperature of the air being 36°, that of the

* I have also frequently, in the course of these experiments, frozen, by means of cooling mixtures, small quantities of sea water in tubes, with a thermometer in the water. When a certain degree of agitation was used, the water generally froze at about 25° or 26°; but when, (as will be seen in the latter part of this paper,) a more considerable vessel of sea water was exposed to congelation, the vessel being quite full and kept at rest, the water was cooled to between 18° and 19° before it became solid. In either case the thermometer uniformly rose to 28°, at the moment of congelation.

water was found to be 33° at the surface ; 30° at the depth of 80 fathoms ; 29° at 200 fathoms ; 28.5° at 400, and 25° at 670 fathoms.* These results are the more singular, as they are at direct variance with those obtained, nearly at the same period, by Lieut. FRANKLIN, in the Polar or Greenland seas, in higher latitudes. It will be seen by the curious and valuable table which Lieut. FRANKLIN has permitted me to annex to this paper, that, with only one or two exceptions, he uniformly found the sea to be sensibly warmer at great depths than near the surface, and that the difference often amounted to four or five degrees. Lieut. BEECHY, one of the officers of the same vessel, and Mr. FISHER, who was on board the *Dorothea*, both of whom made similar observations, have also favoured me with an account of their results, which, as will be seen by a reference to their respective tables, perfectly coincide, in their general import, with those of Lieut. FRANKLIN.

* Captain Ross in his account of a ' Voyage to the Arctic regions,' has himself published some of the results which he obtained respecting the temperature of the sea in Davis's Straits, and Baffin's Bay. Thus in latitude 72.22 , longitude 79 , he found the temperature of the bottom of the sea, at the depth of 1050 fathoms, 28.5° (Appendix, p. lxxxv.). And in latitude 72.23 , having examined the temperature of the sea at the depth of 500, 600, 700, 800 and 1000 fathoms, he found that it gradually decreased from 35° to $28\frac{1}{2}$ (Appendix, page cxxiv.). These differences, though not so considerable as that above related, all concur in establishing the general fact, that the lower strata, in that particular track of the northern ocean, are colder than the surface. The instrument which Captain Ross employed, was a register-thermometer, the indications of which were occasionally compared with the temperature of the mud and earthy fragments of various kinds which he raised from the bottom of the sea, by an appropriate instrument of his own contrivance ; as this mud, both from the quantity raised, and from the manner in which it was confined, retained its temperature for a sufficient length of time not to be materially altered on reaching the surface.

On the other hand, Lieut. PARRY, who had the command of the ship *Alexander*, in Captain Ross's expedition (and is now appointed commander of the second expedition to Baffin's Bay), fully confirms the observations made by Cap. in Ross, and also by Captain SABINE,* on board Captain Ross's ship; so as to place beyond all doubt the fact of Baffin's Bay being colder at the bottom than it is at the surface.†

But although these points may be considered as satisfactorily established, it must be admitted that the various modes

* Captain SABINE has been so obliging as to furnish me with a table containing some of his observations on the subject, which will be found in the Appendix.

† Captain PHIPPS also states in his Journal (Appendix, page 142), that he found the temperature in Baffin's Bay, at the depth of 680 fathoms, as low as 40°, the surface being 55°, and the air 66½°.

Other observers have obtained, in other seas, analogous results. Thus, DE SAUSSURE having examined with great care the temperature of the Mediterranean at various depths, found it in two different places to be 10,6° of REAUMUR's scale, or about 56° FAHR. at the depth of 900 and 1800 feet, the surface being about 71°; and he was induced to conclude that the temperature of the Mediterranean at great depths is uniform, and not likely to be affected by the vicissitudes of the atmospheric temperature, or by changes of season (*Voyage dans les Alpes*, III. § 1351 and § 1391).

M. de HUMBOLDT, whose attention was often directed to this subject, makes the following curious observation. "In the seas of the tropics we find that at great depths the thermometer mark 7 or 8 centesimal degrees (or about 45° FAHR.). Such is the result of the numerous experiments of Commodore ELLIS and of M. PERON. The temperature of the air in those latitudes being never below 19° or 20° (or about 56° FAHR.), it is not at the surface that the waters can have acquired a degree of cold so near the point of congelation, and of the maximum of the density of water. The existence of this cold stratum in the low latitudes is an evident proof of the existence of an under-current, which runs from the poles towards the equator: it also proves that the saline substances, which alter the specific gravity of the water, are distributed in the ocean, so as not to annihilate the effects produced by the differences of temperature." (*Personal Narrative of Travels*, English edition, Vol. I. page 63.)

in which the experiments were made, could not be relied upon as to perfect accuracy.*

It is obvious that these defects in the methods employed, though affecting the precision of the results, and rather tending to render them less striking, could not in the least degree invalidate the general conclusion, that in Davis's Straits, and in Baffin's Bay, the sea, at great depths, is considerably colder than at the surface; while to the east of Greenland, and in rather higher latitudes, the temperature of the ocean follows precisely the opposite law.

These various facts having an obvious and immediate connexion with the density of water under different temperatures, my attention was naturally directed to that circumstance in respect to sea water, which had not yet, I believe, been the subject of direct investigation. It had been long suspected, but was first established by DELUC, and afterwards correctly ascertained by Sir CHARLES BLAGDEN, that water, in cooling towards the freezing point, ceases to contract when its temperature reaches about the 40th degree; but that, on the con-

* Captain Ross, who generally used a register-thermometer, might easily have detected, by a comparative observation, any material error made in ascertaining the temperature of the mud which he brought up by his apparatus; and as he appears to have occasionally availed himself of that mode of checking his observations, we may presume that his results were free from any considerable error. Lieut. FRANKLIN, on the other hand, when he could not reach the bottom, and was therefore unable to make use of my machine, employed that used by Dr. IRVING, consisting of a leaden cylindrical vessel with two valves; a convenient apparatus, but which, as I before observed, is liable to some inaccuracy. He sometimes also used a corked bottle, which he sunk to a great distance from the surface, and by means of which he obtained, doubtless, water from considerable depths; but it was obviously impossible to estimate with exactness the precise depth from which this water was procured, or the change of temperature which it had undergone in traversing the upper strata.

trary, it begins to expand, and continues to do so till it becomes solid, at which moment it undergoes a farther and much more considerable expansion.* The question which I was desirous of ascertaining was, whether the same, or any analogous law, prevailed in regard to sea water.

The mode in which I first attempted to decide this point, was simply by cooling sea water, by means of cooling mixtures, till it reached the freezing point, and ascertaining its specific gravity, at each degree of temperature, as it approached congelation. Researches of this description are liable to a variety of practical difficulties, which I could not altogether overcome by this method, and the results which I obtained, offered slight inconsistencies, which prevented my relying upon their strict accuracy.† Still however they uniformly led me to the conclusion, that the law of greatest specific density at 40° , did not prevail in the case of sea water; but that, on the contray, sea water gradually increased in weight down to the freezing point, until it actually congealed.

Soon afterwards I used another method, which afforded more precise, and, as far as I am able to judge, decisive results. Instead of weighing the water, I measured its bulk, under various temperatures, by means of an appropriate apparatus. A sketch of this instrument (which was executed by Mr. NEWMAN) is given in Plate XII., and an explanation is annexed, which supersedes the necessity of any farther de-

* Philosophical Transactions for 1788, page 143.

† In experiments of this kind it is always necessary to make an allowance for the contraction of the glass vessel, the effect of which is to produce an apparent expansion of the fluid contained in it. There are formulæ for this purpose, and in particular that derived from ROY's experiments, which was adopted in GILPIN's Tables. According to ROY, a vessel of glass of the capacity of 10,000,000, would enlarge, by 1 degree, to the capacity of 10,000,129.

scription. The general conclusion drawn from four experiments, the results of which did not essentially differ from each other, was, that if a vessel filled with sea water of the specific gravity of about 1027, and of any temperature above the freezing point, be gradually and slowly cooled, the water contracts in bulk; and that this contraction continues to proceed, though in a diminishing ratio, till the temperature has reached 22° of FAHRENEIT'S scale. At this point the water appears* to expand a little, and continues to do so till its temperature is reduced to between 19° and 18° , at which point the fluid suddenly expands to a very considerable degree, shooting up with great rapidity, and forcing itself out at the open end of the tube. At the same moment the thermometer rises to 28° , and remains at that point. The liquid is now

* I say *appears*, because the rise of the column, occasioned by the contraction of the glass, may in part account for this effect. It would have been extremely difficult to have estimated this circumstance with precision in the above experiment, because the tube belonging to my apparatus was not perfectly uniform in its bore. But by ascertaining the capacity of a given portion of the tube, as well as that of the bulb of the apparatus, and calculating the contraction produced in the glass by a reduction of four degrees of temperature, I have been able to satisfy myself that the effect arising from this contraction could only produce about one-half of the rise of the column observed in this experiment. So that it can hardly be doubled but that some expansion, however small in its amount, takes place in sea water when cooled from 22° to 18° . But I hope to be soon able to repeat the experiment in a more perfect manner, by a method similar to that employed, for an analogous object, by MM. DULONG and PETIT, and described in their excellent paper on the "Mesures des Temperatures, &c. 1818."

It may also be objected to this experiment, that the bulb has not its interior cooled uniformly, since the surface must be acted upon by the application of cold before the central parts. This is true to a certain extent. But from the great slowness of the experiment (which lasted about three hours at each time), this source of error is in a great degree avoided; and, that the greatest degree of cold actually reached the centre of the vessel, was proved by a nucleus of ice being formed in it, which closely invested the bulb of the thermometer.

found frozen, and in a few minutes the maximum of expansion is obtained. During this congelation the apparatus was never broken, and I satisfied myself by various trials with other vessels, that if a vent, however small, be allowed to sea water at the moment of freezing, the vessel is preserved entire, which, it is well known, scarcely ever happens in the case of common water.*

A singular consequence to be drawn from these experiments seems to be, that, since sea water does not begin to expand till it has been cooled below the point at which it usually freezes, if its congelation were not retarded, it would become solid without undergoing any previous expansion, and the law in question would altogether cease to exist in the case of sea water.

With regard to the singular anomalies of temperature in the Arctic seas, which have given rise to this digression, though some of the facts in question may now be more easily understood, it would be premature, until the observations have been multiplied, and the facts themselves more accurately investigated, to attempt to bring them under any

* The ice thus produced, it should be remembered, is very different from that which forms on the surface of the sea, since the latter parts with its salt in the act of freezing, a separation which can but very imperfectly take place in confined vessels. Accordingly I found the ice produced in this experiment soft and compressible like the water-ice of confectioners.

With regard to the quantity of expansion which sea water undergoes, in confined vessels, at the moment of freezing, I have been able to estimate it with ease, and with sufficient accuracy, by freezing a known weight of water in a phial, connected with an open tube, and ascertaining exactly the proportion of water forced into the tube during congelation. The result of two experiments which agreed perfectly with each other, was, that the expansion of sea water, when passing to the state of ice, is equal to 7.1 per cent of its bulk.

general law, or to explain the phenomena by particular theories.* Why, for instance, two neighbouring and almost contiguous portions of the ocean, placed nearly alike in regard to solar influence, should differ so widely in the temperature of their waters, the warmer strata being, in one case, found lying above the colder, while in the other that order is reversed, appears perfectly unaccountable. Whether, also, this singular circumstance may lead to inferences bearing upon the question now at issue respecting a north-west passage, I shall not presume to decide. But I may be allowed to indulge a hope, that the facts collected in this paper, may assist future inquirers in forming more accurate views of those grand phenomena of nature, in which the navigation of certain seas, the vicissitudes of seasons, and the geological history of the globe are so essentially concerned; or that they may at least be the means of inducing other and abler observers to turn their attention to this interesting subject.

* Count RUMFORD, in one of his Philosophical Essays (Vol. II. Essay VII.), in endeavouring to trace this class of natural phenomena to final causes, was led to some speculations and generalizations on the comparative temperature of the seas, and of large lakes, at their surface and at different depths, and on the relation which these temperatures bear to climate and to human comfort, which, however hypothetical, possess considerable plausibility and interest. Count RUMFORD's general idea was that the uniform temperature of large lakes at great depths, which Dr SAUSSURE found in the Swiss lakes to be constantly between 41° and 42° , was naturally explained from the circumstance since discovered, of water possessing its greatest density at about that temperature; and he conceived that the object of this law of nature was to preserve in winter a store of warmth at the bottom of these lakes, by which their freezing was retarded at the surface, and altogether prevented at a great depth, thus affording a check to the effects of severe winters. With regard to salt water, however, he took it for granted that the law which fixes the greatest density at about 40° , did ~~not~~ prevail; but that, on the contrary, sea water being denser in proportion as it is colder, the coldest strata must occupy the bottom of the sea, while the warmest arising to the surface, serve to moderate the effects of the Arctic cold. He then

§ II. *On the Saline contents of the Waters of different Seas.*

I confined my remarks, in the first part of this paper, to the subject of the specific gravity and temperature of sea water, in various seas and in different latitudes. It remains for me to offer a few observations on the saline contents of these waters.

An accurate analysis of all the specimens which I have noticed in this paper would have been a most laborious, and indeed almost interminable undertaking, which would not have afforded any adequate object of curiosity or interest. All that I aimed at, therefore, was to operate upon a few of the specimens, so selected as to afford a general comparison between the waters of the ocean in distant latitudes and in both hemispheres, and to enable me also to ascertain whether particular seas differed materially in the composition of their waters.

For this purpose, availing myself of the experience I had obtained, in former inquiries of this kind,* respecting the

supposed that the colder and heavier strata would form sub-marine currents, constantly moving from the vicinity of the poles towards the equator, and occasioning upper and warmer currents precisely in an opposite direction. It is obvious that this theory, though capable of explaining some of the phenomena above mentioned, cannot apply to those of an opposite nature, also related in this paper. Yet these may possibly depend upon peculiar and local causes; and I cannot omit to observe, that M. DE HUMBOLDT, in the work already quoted, entertains notions of an exchange constantly going forward between the waters of the Polar regions and those of the Equatorial seas, which bear considerable analogy to those of Count RUMFORD, and cannot fail to give them additional weight.

* See an 'Analysis of the Brighton Chalybeate,' published in Dr. SAUNDERS'S Treatise on Mineral Waters, 1805. Also 'An Analysis of the Waters of the Dead Sea and

difficulty, and indeed the impossibility, of analyzing complex solutions of saline substances with a view to obtain a precise and certain knowledge of the state of combination in which the salts exist in these solutions, I contented myself with ascertaining, first, the proportions of saline matter yielded by a given quantity of each water, and afterwards, the proportions of acids and earths contained in these respective waters; thus presenting data which are quite divested of theoretical views, and from which the composition of those waters may at any time be inferred in the way which may be deemed most eligible.

It has been long known that the principal salts contained in sea water are muriate of soda and muriate of magnesia, and that it contains also sulphuric acid and lime. But whether these ingredients existed in the form of sulphate of soda, or of sulphate of lime, or muriate of lime, or sulphate of magnesia, was more or less a matter of conjecture, as the different states of binary combination which they assume, are modified during evaporation by the different degrees of solubility which the salts possess, and are liable to be influenced by heat and concentration, the very processes which are used in attempting to resolve the question. These difficulties have been ably discussed by Dr. MURRAY,* whose reasonings and experiments on the subject have given great plausibility to the doctrine which he has proposed, according to which the salts contained in sea water are supposed to be :

River Jordan ;' *Philos. Trans.* 1807. And ' *An Analysis of an Aluminous Chalybeate Spring in the Isle of Wight* ;' *Geolog. Trans.* Vol. I. 1811.

* See ' *An Analysis of Sea Water* ' read in 1816, and published in the *Edinburgh Transactions*, Vol. VIII. and also a ' *Formula on the Analysis of Mineral Waters*, ' printed in the same volume.

Muriate of soda,
Muriate of magnesia,
Muriate of lime,
Sulphate of soda.

Still however, it must be admitted that a degree of doubt remains respecting the mode in which the sulphuric acid is combined, and that we can only pronounce with certainty upon the proportions of acid and base taken singly, as I have explained above. My experiments, therefore, were confined to the following points.*

1st. To ascertain the quantity of saline matter contained in a known weight of the water under examination, desiccated in a uniform and well defined mode; and to compare it with the specific gravity of the water.

2ndly. To precipitate the muriatic acid from a known weight of the water, by nitrate of silver.

3dly. To precipitate the sulphuric acid by nitrate of barytes, from another similar portion of water.

4thly. To precipitate the lime from another portion of water, by oxalate of ammonia.

5thly. To precipitate the magnesia from the clear liquor remaining after the separation of the lime, which is best effected by phosphate of ammonia, or of soda, with the addition of carbonate of ammonia.

The soda, by this method, is the only ingredient which is not precipitated, and which therefore, can only be inferred

* It is but just to mention that I received, in this part of the inquiry, much valuable aid from Mr. WILSON, who has many years acted as assistant, to my colleagues and myself, in the Chemical Theatre of Guy's Hospital.

by calculation. But if the processes are conducted with sufficient care, this mode of estimating the proportion of alkaline muriates is susceptible of great accuracy, as I had an opportunity of ascertaining by some comparative experiments which I related at full length in the analysis of the waters of the Dead Sea.*

The whole of the results obtained by this mode of investigation, has, for the sake of brevity, been condensed into a table which is annexed to this paper, and upon which it is unnecessary to detain the Society by any farther comment. It will be seen by this table that, with the exception of the Dead Sea, and of the Lake Ourmia,† which are mere salt ponds, perfectly unconnected with the ocean, all the specimens of sea water which I have examined, however different in their strength, contain the same ingredients all over the world, these bearing very nearly the same proportions to each other; so that they differ only as to the total amount of their saline contents.‡

* In devising the above method, I followed, step by step, the plan which I had myself pointed out, and actually used, in various analyses, and particularly in that of the Dead Sea, and of an aluminous chalybeate, in the Isle of Wight, as may be seen by a reference to these papers. It is satisfactory to observe that Dr. MURRAY adopted, several years afterwards, from considerations of the same kind, a mode of proceeding precisely similar, and indeed that he proposed in a subsequent paper, a general formula for the analysis of mineral waters, in which this method is pointed out as likely to lead to the most accurate results. And this coincidence is the more remarkable, as it would appear, from Dr. MURRAY not mentioning my labours, that they had not at that time come to his knowledge.

† I had only between 2 and 300 grs. of water from this curious lake, which is so nearly saturated, that it begins to deposit crystals the moment that heat is applied to it. Though it contains no lime, it yields about 20 times as much sulphuric acid, and six times as much muriatic acid as sea water does, as may be seen by the annexed table. Dr. WOLLASTON has also detected traces of potash in this water.

‡ The Yellow Sea, in the Chinese ocean, has some peculiarities which deserve to

It would hardly be consistent with the plan which I have followed in this inquiry, to enter minutely upon the analysis of the waters of individual seas, since, instead of dwelling on analytical details, I have rather aimed at presenting an extensive and comparative view of the subject, for the purpose of drawing certain general inferences. Yet as my experiments were made with care, and appear from their consistency with each other, to justify some degree of confidence as to the accuracy of their results, it may not be out of place to select from the above table some individual water, with a view

be noticed. The smell of the specimens put into my hands by Captain HALL, was exceedingly *hepatic*, like that of a strong solution of sulphuretted hydrogen, and this water formed with silver a black precipitate. It was clear and transparent; but had a greenish yellow colour. Nitric acid made it milky, and precipitated sulphur from it. When boiled it gave out sulphuretted hydrogen gas, and deposited a yellowish sediment, which proved to be carbonate of lime, in the proportion of 0.7 grs. for 500 grs. of the water, and without any sulphur being mixed with the sediment. The interior of the bottles was found blackened, so as to render the glass quite opaque; but the black film was easily wiped off, and the glass was not permanently stained. After evaporating the water to dryness, the residue dissolved readily in water, with the exception of the carbonate of lime above mentioned, and the solution now precipitated silver perfectly white. In other respects the saline contents of this water did not differ from those of other seas. Its specific gravity was low (1022.9), but the salts, with the exception of a small deficiency in the magnesia, were the same as usual. The water was first put by Captain HALL into a green-glass bottle; but it was, some months afterwards, transferred into several white-glass phials, having glass stoppers, all of which exhibited the appearance above described. There is something in this developement of sulphur in sea water which is by no means well understood. Of two specimens brought from the same spot, and by the same individual, I have sometimes observed that the one had a smell of sulphuretted hydrogen, while the other was perfectly free from it. In the former case the cork was commonly blackened and decayed. I therefore suspect that in some instances the cork gives the impulse to the formation of sulphuretted hydrogen; but in others, and probably in the Yellow Sea, this change is likely to be owing to the presence of some vegetable or animal matter, and its gradual action on the saline water.

to show how the various statements which it contains may be reduced to the form in which analytical results are usually expressed.

Thus, for instance, if we select the water marked No. 27, which was taken up nearly in the middle of the North Atlantic, and the specific gravity of which was 1028.86, 500 grs. of this water yielding 21.3 grs. of saline matter, dried at 212°, we shall proceed in the following manner :

The muriate of silver obtained from 500 grs. of the water being 42 grs., 100 grs. of which are equal to 19.05 of dry muriatic acid, the 42 grs. of luna cornea will be equal to 8 grs. of muriatic acid.

The sulphate of barytes obtained from a similar portion of water being 3.85 grs. dried at 212° = 374 grs. dried at a red heat,* 100 grs. of which contain 34 grs. of sulphuric acid, the quantity of dry sulphuric acid in 500 grs. of the water will be $(100 : 34 :: 3.74 : 1.27)$ 1.27 grs.

The oxalate of lime, from a similar portion of water, being 0.8 grs. dried at 212°; and 100 parts of oxalate of lime so dried being = 0.314† of pure lime, the quantity of lime in 500 grs. of the water will be 0.314 grs.

The phosphate of magnesia being 2.7 grs., 100 of which contain 40 of magnesia, the quantity of magnesia in 500 grs. of the water will be 1.08 grs.

It appears, therefore, that the quantities of acids and earths

* I found by a careful experiment, made for the express purpose, that 100 grs. of sulphate of barytes, dried at 212°, were reduced by a red heat to 97.2 grs.

† I obtained this result from a direct experiment, in which 24 grs. of ignited muriate of lime = 12.24 grs. of pure lime, gave 31.2 grs. of oxalate of lime dried at 212°. Therefore, $31.2 : 12.24 :: 100 : 39.23$.

contained in 500 parts of this water, and estimated in their uncombined state, are as follows :

Muriatic acid,	-	-	8 grs.
Sulphuric acid,	-	-	1.27
Lime, -	-	-	0.314
Magnesia, -	-	-	1.08

It now remains to estimate, from the above data, the compound salts contained in the water, according to their most probable state of combination as before explained ; and to infer the quantity of soda belonging to the same portion of the water, a question which cannot well be ascertained by a direct process. This will be effected in the following manner.

Muriate of lime is known to consist of 51 parts of lime, to 49 of muriatic acid.* Therefore the above 0.314 gr. of lime = 0.302 of muriatic acid = 0.616 gr. muriate of lime, free from water.

Sulphate of soda, in its dry state, consists of 56 parts acid, to 44 soda ;† and therefore the above 1.27 gr. of sulphuric acid = 1.01 soda = 2.33 grs. dry sulphate of soda.

Muriate of Magnesia, in a state of dryness, consists of 58.09 parts of muriatic acid, to 44.91 of magnesia.‡ Therefore the 1.08 of magnesia are equivalent to 1.497 of muriatic acid ($44.91 : 58.09 :: 1.497$) = 2.577 of dry muriate of magnesia, in 500 grs. of the water.

* Scale of Chemical Equivalents.

† Scale of Chemical Equivalents ; and 100 parts of crystallized sulphate of soda consists of, sulphuric acid 24.5 ; soda 19.5 ; water 56. The above 2.33 grs. therefore would amount to 5.3 grs. crystallized sulphate of soda.

‡ Scale of Chemical Equivalents.

We are now enabled to estimate the quantity of muriate of soda. For the quantities of the muriatic acid already assigned to the earthy bases, being as follows, viz :

in combination with lime	-	0.302 grs.	} 1.799 grs., and
----- with magnesia	-	1.497	

the total quantity of muriatic acid being 8 grs. there will remain 6.2 grs. of the acid in combination with soda. But dry muriate of soda consists of 46.6 parts of muriatic acid to 53.4 of soda;* and consequently the 6.2 grs. of muriatic acid = 7.1 grs. soda = 13.3 grs. muriate of soda.

It appears therefore that the saline and earthy substances contained in 500 grs. of the specimen of sea water under examination, taken in the uncombined state, are

Muriatic acid	-	8 grs.
Sulphuric acid	-	1.27
Lime	- -	0.314
Magnesia	-	1.08
Soda	- -	8.11†

18.774

And the same saline ingredients, in their state of combination, and supposed free from water, will be

Muriate of soda	-	13.3
Sulphate of soda	-	2.33
Muriate of lime	-	0.616
Muriate of magnesia		2.577
		<hr/>
		18.823 grs.

* Scale of Chemical Equivalents.

† Viz. 1.01 with the sulphuric acid, and 7.1 with the muriatic.

This total amount, it may be observed, does not exactly correspond with the saline residue of 21,3 grs. obtained by evaporation from 500 grs. of the water; but it should be remembered that this residue was dried at 212° only, which, with some salts, produces a considerable difference. I thought it important to ascertain the amount of this difference by direct experiments; and I found that 100 grs. of muriate of lime dried at 212°, were reduced by ignition to 61,9; so that if 100 grs. of muriate of magnesia, dried at 212°, be supposed to be brought to a state of perfect dryness, they will be reduced to 52 grs. As to the muriate of soda and sulphate of soda, when well dried at 212°, they lose no sensible weight by being ignited.

Upon making due allowance for the moisture contained in the two earthy muriates, according to the estimates just mentioned, we shall find the above result altered as follows :

Muriate of soda	-	-	13,3
Sulphate of soda	-	-	2,33
Muriate of lime	-		0,975
Muriate of magnesia	-		4,955

21,460

Which result closely corresponds with the saline residue obtained by evaporation, which was 21,3 grs.

It remains for me in concluding this paper, to communicate to the Society an interesting fact on the composition of sea water just discovered by Dr. WOLLASTON, and which it is no small gratification to me to think that the present inquiry has been the accidental means of bringing to light. As I was beginning the chemical part of this investigation, Dr. WOL-

LASTON put the question to me, whether it was not probable that traces of potash might be found in sea water? I answered in the affirmative, and thought the fact well worthy of investigation;* but as no one could be better qualified than the Doctor himself to put his own suggestion to the test of experiment, I supplied him with sea water, and begged of him to favour me with his results, which he has just communicated to me in a note to the following effect:

“The expectation which I expressed to you that potash would be found in sea water as an ingredient brought down by rivers from the decay of land-plants, is now fully confirmed by experiments on waters obtained from situations so remote from each other as to establish its universality.

“There is no difficulty in proving the presence of this ingredient by muriate of platina. For though the triple muriate of platina and potash is so soluble that this reagent causes no precipitate from sea water in its ordinary state, yet when the water has been reduced by evaporation to about $\frac{1}{8}$ th part, so that the common salt is beginning to separate by crystallization, the muriate of platina then causes a copious precipitate.

“If this precipitate be mixed with a little sugar and heated, the platina is reduced, and muriate of potash may be separated from it by water; and the nature of its base shown by its yielding crystals of nitrate of potash with nitric acid.

“I evaporated a pint of the water which you sent me (marked No. 9, specific gravity 1026.22) taken up by Captain Ross in

* I, in my turn, put the question to Dr. WOLLASTON whether it was not probable that minute quantities of all soluble substances in nature might be detected in sea water?

Baffin's Bay, from the depth of 80 fathoms, latitude $36^{\circ}32'$, longitude $76^{\circ}46'$ west. When this had been reduced to about $\frac{1}{250}$ th part, I drained the liquor from the salt that had formed, which I also washed with a little water, and by adding muriate of platina to the drained liquors, I had a yellow precipitate which weighed 12,4 grains.

As the fluid poured from this precipitate measured $\frac{3}{4}$ of a fluid ounce, I estimate that this would retain in solution about three grains of the triple muriate, and hence the whole amount must be taken at 15,4, which by former experiments I consider as equivalent to about 6,4 sulphate of potash,* or 3,5 potash.

Now, since the pint of water weighed about 7520 grains, $\frac{6.4}{7520}$ gives the proportion of potash about $\frac{1}{1200}$; but the quantity of mere potash is less than $\frac{1}{2000}$ th part of sea water at its average density."

* Dr. WOLLASTON thinks it probable that the potash exists in sea water in the state of sulphate.

TABLE V. Presenting a Synthetic View of the results obtained from the Analysis of different Seas; the quantity of water operated upon being in every instance supposed to be 500 grains.

Description of the Specimens.	Specific Gravity.	Residue of Evaporation of 500 grains of water.	Muriate of Silver.	Sulphate of Barytes.	Oxalate of Lime.	Phosphate of Magnesia.	Total of Precipitates from 500 grs. of water.	Observations.
Arctic Ocean. Spec. 1.	1027.27	Grains. 19.5	Grains. 39.7	Grains. 3.3	Grains. 0.85	Grains. 2.7	Grains. 46.55	The quantity actually operated upon was 500 grains.
Arctic Ocean. Spec. 12.	1019.7	14.15	27.9	2.4	0.7	1.8	32.8	From surface. Quantity operated upon 500 grs.
Arctic Ocean. Spec. 67.	1002.35	1.75	3.2	0.1	0.05	0.03	3.37	Sea ice water; Coast of Spitzbergen. Operated on 500 grains.
Arctic Ocean. Spec. 14.	1027.05	19.3	38.9	3.25	0.95	2.9	46.	From a depth. Operated on 500 grs.
Equator. Spec. 35.	1027.85	19.6	40.3	3.7	0.9	3.1	48.	From surface. Operated on 500 grs.
South Atlantic. Spec. 41.	1028.19	20.6	40.4	3.75	1.0	3.2	48.3	Operated on 250 grs.
White Sen. Spec. 58 and 59.	1022.55	16.1	31.8	3.0	0.6	2.2	37.6	Operated on 500 grs. but evaporated only 250 grs.
Black Sea. Spec. 56 and 57.	1014.22	10.8	19.6	1.95	0.55	1.5	23.6	Operated upon 500 grs. for the earths; but upon only 250 for muriate of silver and evaporation of the water.
Baltic. Spec. 60.	1004.9	3.3	7.	0.7	0.2	0.6	8.5	Operated upon 250 grains. All the precipitates were slightly tinged by some vegetable or animal matter.
Sea of Marinora Surface. Spec. 53.	1020.28	14.11	28.4	2.65	0.4	2.35	33.8	Entrance of Hellespont. Surface. Operated on 500 grs., except for muriate of silver.
Sea of Marinora. Bottom. Spec. 52.	1028.19	21.	40.4	3.55	0.9	3.2	48.05	From the bottom. A little carbonate of lime was deposited during evaporation; but none from the water at the surface. Operated on 500 grs.
Middle of North Atlantic. Spec. 27.	1028.86	21.3	42.	3.85	0.8	2.7	49.35	Operated on 250 grs. for evaporation of the water and precipitation of muriate of silver. 500 for the other salts.
Yellow Sea. Spec. 48.	1022.91	16.1	32.9	1.35	0.75	2.2	37.2	During concentration deposited carbonate of lime. (See note, p. 195.) The water was yellowish, and had an exceedingly strong hepatic smell. Proportion of magnesia rather smaller than common. Operated on 500 grs.
Mediterranean. Spec. 51.	1027.3	19.7	38.5	3.6	0.8	3.0	45.9	From Marseilles, and therefore rather weak, from the vicinity of rivers. Operated on 100 grs. for evaporation and muriate of silver; and 250 for the other salts.
Dead Sea.	1211.	192.5	326.4	0.5	9.78	55.5	584.68	Philosophical Transactions, 1807.
Lake Ourmia, in Persia.	1165.07	111.5	237.5	66.0	0.	10.5	425.5	Specimen brought by the traveller Brown. Operated on 100, and 50 grs.

GENERAL OBSERVATIONS.

In the above experiments, the residues were dried as follows, viz. The residue obtained from the water by evaporation, was thoroughly dried at a boiling heat in a water-bath, till it entirely ceased to lose weight. The muriate of silver was heated to incipient fusion; the sulphate of barytes and oxalate of lime were dried at a boiling heat; and the ammoniaco-phosphate of magnesia, was heated to redness. No filters were used. The precipitates were washed, dried, and weighed, in the same glass capsules in which they were formed, with the exception of the magnesian salt, which was heated to redness by means of the blow-pipe, in a very thin and small platina crucible.

TABLE VI. Showing the differences in Temperature of Water from a depth or bottom, and at the surface, observed on board His Majesty's Brig TRENT, in the Arctic Seas; by Lieutenant FRANKLIN.

Date 1818.	Latitude North.	Longitude East	Water obtained either from a given depth or bottom, as expressed.	Its Temperature.	Temperature of Water at surface at same time.	Temperature of Air.	Remarks as to the Situation of the Vessel with respect Land or Ice.
May 26	76° 48'	12° 26'	Depth, 700 fathoms.	43°	33°	29°	The ice in small detached pieces around the vessel. The land of Spitzbergen distant 6 or 7 leagues. The temperature of the water obtained, was not tried until the bottle was taken below into the cabin, to which circumstance I think this extraordinary difference of temperature from that of the surface is to be attributed.
June 20	79° 58'	11° 25'	Bottom. 24 fathoms.	31	31½	30	Vessel closely beset by ice.
June 21	79° 56'	—	Bottom. 19 fathoms.	31	30	30	Ship surrounded by ice.
June 22	80.	—	Bottom. 33 fathoms.	31	30	30	Surrounded by ice, not far distant from land.
June 23	79° 59'	10° 12'	Bottom 21 fathoms.	32½	31½	30	Beset in ice, close to the land.
June 25	79° 51'	10	Bottom. 60 fathoms. 17 fathoms.	34 34	33 33	34 34	In open water, near to the land. Clear of ice, about 6 miles from land.
June 26	79° 44'	9° 33'	Bottom. 15 fathoms. 34 fathoms.	34 34	34 34	35 31	In clear open water, some miles from the margin of the ice. Near to the land.
June 27	79° 51'	10	Bottom 72 fathoms.	34½	34	36	Detached pieces of ice near to the vessel.
June 29	79° 51'	10° 18'	Bottom. 17 fathoms. 19 fathoms.	34 34	34 34	39 37	Near to the land, between two islands.
July 6	79° 48'	10° 15'	Bottom. 34 fathoms.	34½	34	36	Near to the land, passing between two islands.
July 8	80° 20'	11° 30'	Bottom. 120 fathoms.	36	33	35	Closely beset in ice—about 11 or 12 leagues from land.
July 8 P. M.	80° 20'	11° 30'	Bottom. 130 fathoms.	36½	31½	33	Closely beset in ice—muddy bottom
July 9 P. M.	80° 26'	11° 38'	Bottom clay, 120 fathoms. 110 fathoms.	36 35½	31 30½	35	Beset as before—about the same distance from the nearest land.
July 10	80° 19'	11° 24'	Bottom. 119 fathoms.	36	32	—	Closely surrounded by ice.
July 11	80° 22'	10° 30'	Bottom. 120 fathoms.	36	32	40	Surrounded by ice—muddy bottom.
July 12	80° 20'	11° 7'	Bottom. 145 fathoms.	35½	32	36	Surrounded by ice—muddy bottom.
July 13	80° 22'	11.	217 fathoms. Bottom.	37	32½	—	Rocky bottom.
July 13	80° 22'	11° 2'	235 fathoms. Bottom.	35½	32	40½	Surrounded by ice—rocky bottom
July 13	80° 23'	10° 55'	Bottom. 237 fathoms. Muddy.	35½	31½	40	Beset amongst ice about thirty miles from land. A specimen has been forwarded to Dr. Marcet.
July 14	80° 26'	—	Bottom. 233 fathoms. Muddy.	35½	32	39	Surrounded by ice.
July 15	80° 27' 80° 28'	10° 20' 10° 20'	Bottom. 198 fathoms. 185 fathoms. Mud.	36 36½	32 32½	38	Beset amongst ice.
July 16	80° 26'	11° 25'	Bottom. 173 fathoms. Clay and mud.	36½	32	39	Closely surrounded by ice about 30 miles from land.
July 17	80° 27'	11	Bottom. 285 fathoms.	35½	34	—	Ice very closely besetting the vessel.
July 18	80° 26'	10° 30'	Bottom. 305 fathoms. Muddy.	36	32½	36	
July 19	80° 24'	11° 14'	Bottom. 109 fathoms.	36½	31½	41	The ice closely surrounding the vessel.
July 20	80° 21'	10° 12'	Bottom. 188 fathoms.	35½	32½	34½	More open water than usual, distance from land 1½ leagues.
July 21	80° 14'	12° 19'	Bottom. 95 fathoms.	35½	32½	41½	Surrounded by ice.
July 22	80° 15'	11.	Bottom. 83 fathoms.	35½	31	41	Beset by ice.
July 23	80° 15'	11° 36'	Bottom. 73 fathoms.	36½	32½	37	The ice opening a little.
July 25	80° 16'	11.	Bottom. 94½ fathoms.	36	32½	34	The water more open than for the last day.
July 26	80° 20'	11° 25'	Bottom. 55 fathoms.	36	32	36	Surrounded by heavy ice.
Sept. 10 P. M.	75° 14' 75° 14'	3° 53' 3° 53'	Depth. 756 fathoms. 756 fathoms.	36 36	35 36	37	In open water—several miles distant from the ice.
Sept. 24	66° 35'	5° 33'	Depth. 260 fathoms.	41½	43	44½	A bottle of this was preserved, and is completely in the open ocean, 500 miles from the ice.

TABLE VII. *Temperature of the sea at the surface, and at different depths; as observed by Lieutenant BEECHEY, on board the TRENT, in the late voyage to the Arctic Seas.*

Date.	Latitude. North.	Longitude. East.	Depth.	Temperature of bottom.	Temperature of surface.
			Fathoms.	°	°
May 26	76. 48	12. 26'	700	43	33
June 21	79. 56	11. 26	24	31	31.5
22	79. 58	11. 14	30	31	30
25	79. 52	9. 57	60	34	32
26	79. 44	9. 34	15	34	33
July 4	79. 49	11.	35	34.5	34.1
7	80. 16	11. 5	34	34.5	34.5
9	80. 23	9. 50	120	36	30.3
12	80. 21	11. 11	140	36.5	30.5
13	80. 23	11. 3	237	37	32
15	80. 27	185	36.3	32.7
16	80. 27	11. 5	173	36.5	32
17	Ditto	Ditto	200	35.5	32.5
18	Ditto	Ditto	331	35	32
19	80. 25	11. 14	103	36.5	31.3
20	80. 24	10. 5	108	35.5	31.5
21	80. 13	11. 14	95	35.3	32
Sep. 24	66. 38	5. 44	260	41.5	43.5

We invariably found the temperature of the water increase with a southerly gale, and decrease as we approached the ice. At Spitzbergen, in August, the flood tide which came from the southward, was 3° warmer than the ebb; the former being 37°, the latter 34°.

TABLE VIII. *Temperature and specific gravity of sea water at the surface, and at certain depths, as ascertained by Mr. FISHER, on board the DOROTHEA, during the late voyage to the Arctic Seas.*

Temperature of different depths compared with the surface.*								
Date.	Lat.	Long.	Below the surface.			Surface at the same time.		Situation of the Ship.
			Depth in Fathoms.	Sp. Gr.	Temp.	Temp.	Sp. Gr.	
July 1819.	Between 79.° 50' and 80. 14	About 11.° 30 E.	40	1.0275	35.5	31.8	1.0267	About 10 leagues distant from Spitzbergen. The ship in general closely beset with ice.
			60	1.0275	36	32	1.0112	
			100	1.0274	36.3	32	1.0106	
			124	1.0279	36.7	33.5	1.0263	
			140	1.0279	36.5	32	1.0255	
			188	1.0281	42.5	33	1.0245	
			304	1.0282	39	31	1.0086	

* The above are the means between those observations most to be relied on. The Specific Gravities were taken with great care while the ship was beset in ice, and had no motion, with an hydrostatic balance, made for me by NEWMAN, of Lisle-street.

TABLE IX. *Representing the Temperature at the Surface in a series of latitudes, both going out and coming home ;* such as observed by Mr. FISHER.*

	Lat.	Temp.	Temp.	Sp. Gr.	
		°	°		
	60	46.7	50.9	1.0276	
	61	45.5	49.2	1.0276	
	62	45.6	46.1	1.0275	
	63	45.3	44.2	1.0276	
	64	45.	43.1	1.0275	
	65	44.9	42.7	1.0275	
	66	44.8	45.3	1.0275	
	67	44.7	45.3	1.0274	
	68	42.8	47.3	1.0275	
	69	40.5	42.6	1.0275	
	70	39.2	40.9	1.0275	
	71	37.9	36.8	1.0276	
	72	36.7	36.2	1.0276	
	73	38.8	33.6	1.0277	
	74	38.6	35.9	1.0275	
	75	37.5	35.8	1.0275	
	76	35.9	35.6	1.0274	
	77	31.5	33.9	1.0273	
	78	30.9	36.4	1.0272	
	79	31.9	36.6	1.0267	
	80	32.7	32.7	1.0267	
	81			1.0058	

* Each of these results is a mean between all those taken between each degree of latitude and the succeeding one; thus the temperatures annexed to 65°, which are 44.9° and 42.7°, are means between the observations taken between 65° and 66°. The specific gravities are means of those taken both going out and coming home; for the differences of specific gravity (which, when the ship was in motion, was observed by an Hydrometer, by TROUGHTON) were probably in a great degree occasioned by the unavoidable errors of observation.

TABLE X. *Comparative Temperatures of Sea Water at the Surface and at certain Depths, as ascertained on Board the ALEXANDER, during the late Voyage to Baffin's Bay, by Lieut. PARRY.*

Day of the Month.	Latitude. North.	Longitude. West.	Depth in Fathoms.	Temperature of Water.		Temperature of Air in the Shade.
				Below.	Surface.	
1818.						
June 1	63. 50'	55. 30'	145	32°	36°	35½°
July 18	74. 50	59. 30	197	29½	32	37
Aug. 14	75. 56	66. 31	200	30½	32 {	36
Aug. 22	76. 33	77. 10	422	29½		
Aug. 24	76. 22	77. 38	102	29½	32	36
			100	30½	31½ {	33
			240	29½		
Aug. 25	76. 8	78. 31	54 } Bottom.	29½	32	31½
Aug. 29	74. 58	77. 42	170	31	36	34
Sept. 1	73. 38	77. 19	125	30½	35	36
Sept. 5	72. 39	74. 30	190	30½	35	39
Sept. 6	72. 22	73. 06	246	30	36	41
Oct. 27	61. 48	1. 52	473	47	49	50

W. PARRY.

TABLE XI. *Comparative Temperatures of the Sea, at the Surface and at certain Depths, as ascertained on Board the ISABELLA, during the late Voyage to Baffin's Bay, by Captain SABINE.*

Date.	Lat. North.	Long. West.	Depth. Fath.	Temperature.			Remarks.
				Below.	Surface.	Air.	
May 23	59°	44°	80	37°	39°	40°	No soundings, deep sea.
Aug. 3	75. 52	63	415	29	34	38	Soundings in 430 fathoms. Soundings in 450 fathoms, mud.
14	75. 50	66	422	29½	32	38	
			200	30½	32	38	
24	76. 35	78	240	29½	31½	33	Soundings in 56 fathoms. Soundings in 170 fathoms.
			100	30½	31½	33	
			54	29½	32½	31½	
25	76. 8	78. 21	170	31	36	34	No soundings.
29	74. 59	76. 37	235	29½	36½	37	Soundings in 190 fathoms.
30	74. 4	79	190	30½	35	35½	No soundings.
Sept. 5	72. 37	74. 6	246	30	36	37	Soundings in 1000 fath.
6	72. 23	72. 55	1000	28½	35	33	Soundings in 750 fathoms.
7	72. 16	71. 18	100	30	33	35	
19	66. 50	61	200	29	33	35	
			400	29	33	35	
			680	25½	33	35	Soundings in 370 fathoms.
26	65. 50	59. 30	310	29	34	36	
Oct. 4	60	58	900	35½	40	37	No soundings.
27	61	7	470	47	49½	50½	No soundings.

EDWARD SABINE.

EXPLANATION OF PLATE XI.

CC. The brass cylinder.

W. The valves ; the one in dotted lines opening upwards.

T. String connecting the two valves, so that they open and shut together.

D. A string fastened at one extremity to the valve in F, passing over the three pulleys PPP, and having a weight suspended at its other extremity W, so that the weight keeps the valves forcibly open.

B S. Springs pressing on the back of the valves, in order to close them forcibly when the weight is removed.

SS. Side springs, also tending to close the valves.

LS. Lock-spring, or catch, to keep the valves fast when closed.

A. The wire by which the machine is fastened to the line.

N. B. The machine is here represented in its natural dimensions ; but it would answer the purpose much better if it were made three or four times larger, and if its weight were proportionally increased.

EXPLANATION OF PLATE XII.

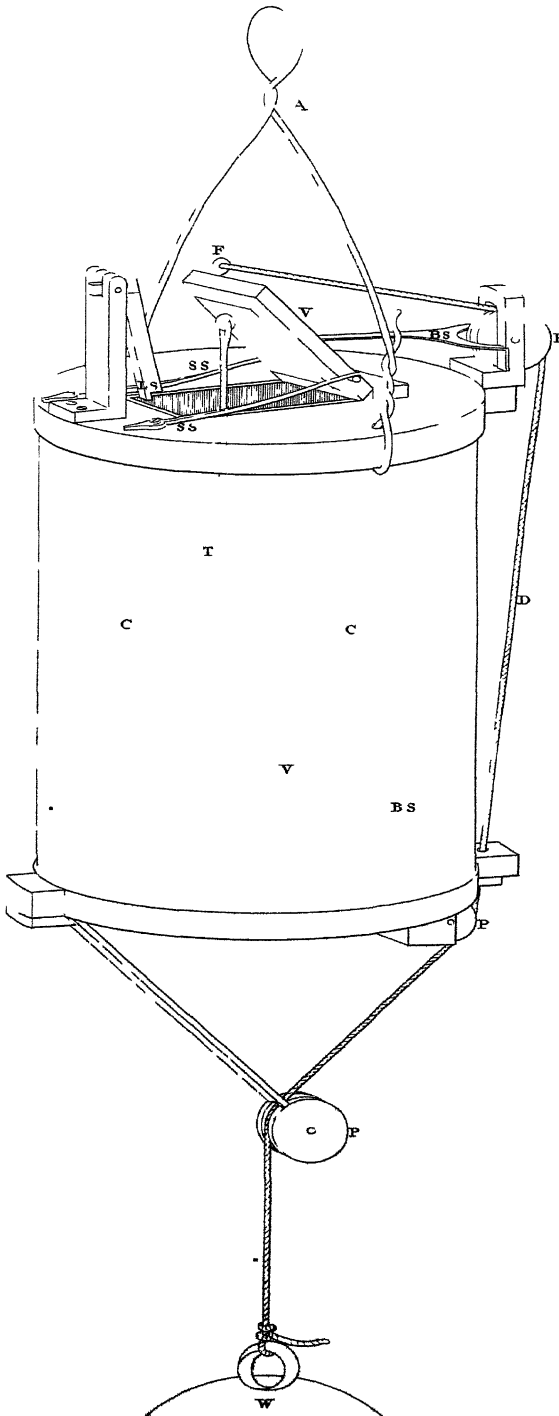
Fig. 1. The principle of this improved machine is essentially the same as that delineated in Plate XI, the valves VV, being kept open by means of a weight W, and closing themselves when the weight reaches the bottom.

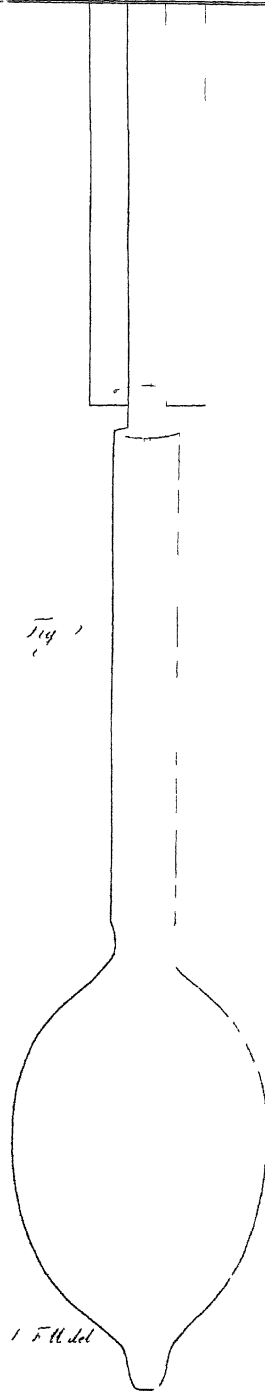
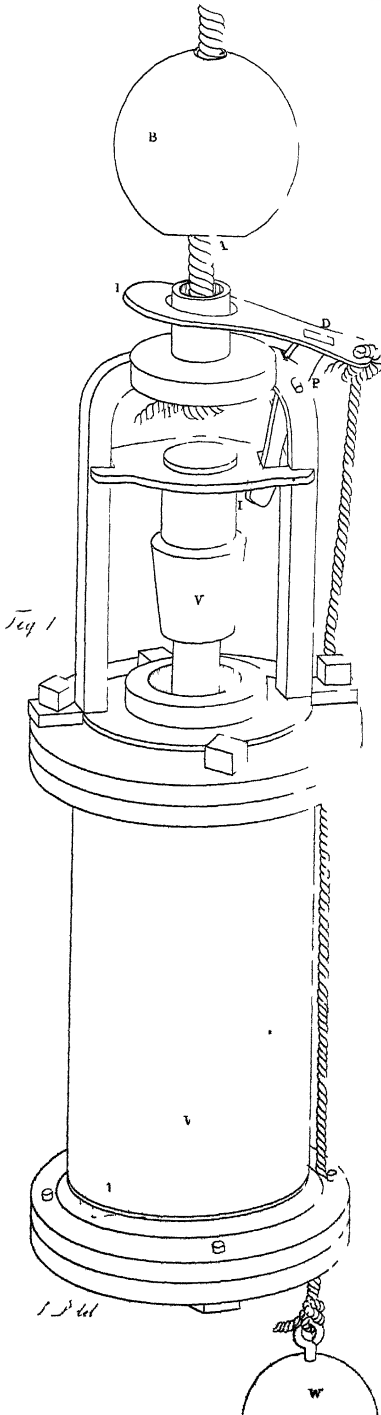
The valves in this machine are made of solid brass, and they fall by their own weight, so as to close the cylinder, the moment that the square FDE, which turns freely upon a pivot in P, is depressed in E, where it preponderates, the piece *cc*, which supports the valves, thus becoming unhooked from the recession of the hook, or clicket, in F. This may be effected in two ways; either by the weight W no longer pressing on the square in F, so as to keep it fast in its place, and therefore suffering it to recede, so as to disengage *cc*; or by letting down along the line a weight B, that shall fall upon DE along the rope A, and disengage the valves by the jerk it occasions. This constitutes the improvement by which water is now expected to be raised from any given depth, as well as from the bottom.

Fig. 2. This figure does not require any particular references. It represents the instrument in its natural size, which simply consists in a glass bulb of moderate thickness, capable of holding 844,6 grains of distilled water, with a neck or tube issuing from it, and containing a delicate mercurial thermometer, the elongated bulb of which is represented in dotted lines in the centre of the large bulb. To the end of this neck (the diameter of which is near half an inch), a long tube having rather a small bore, is ground air-tight, and

a scale of paste-board is fixed to it, in order to record the results of the experiment.

The bulb and neck being then entirely filled with sea water, and the tube fitted on, the fluid is thereby forced up into it to a certain height, which is marked on the scale. The bulb is now enveloped in cotton-wool, or any other bad conductor, and placed in a small jar, and this jar is immersed into a cooling mixture. The fluid is soon seen descending in the tube in proportion as the thermometer descends; and the gradual condensations of the water in the latter part of the experiment, such as they really occurred, may be seen marked on the scale. The level of the fluid in the tube is represented opposite No. 26, 25, 24, and 23, at which temperatures it remains stationary; and it then possesses the greatest specific density which sea water can attain.





XIII. *An account of the fossil skeleton of the Proteo-saurus.*
By Sir EVERARD HOME, Bart. V. P. R. S.

Read March 4, 1819.

IN the year 1814, the skull and vertebræ of this fossil skeleton were first described in the Philosophical Transactions; and so much was the attention of the public called to the subject by that account, and so many specimens were brought under my observation, that in the year 1816, I was enabled to make many valuable additions to my former paper. In 1818, I laid before the Society the description of bones not before met with; and since that time, through the kindness of Mr. DE LA BECHE, and Colonel BIRCH, I have procured materials, which put it in my power to describe nearly the complete skeleton, and to correct any errors, which the imperfect state of the first specimens had led me to commit.

One of these errors was, a belief that the orifice immediately before the margin of the orbit was natural, as it occurred in every specimen of the skull I had met with, five or six in number; but Colonel BIRCH has shown me a portion of a skull of very large dimensions, in which the nasal bones are perfect, and no such orifice is seen; so that the aperture described and delineated in my first paper, is the effect of injury the bones have sustained. A drawing of this skull is annexed, half the size of the original specimen. The drawing is made by Mr. CLIFT.

In a specimen in the possession of Mr. DE LA BECHE, the bones of the sternum are met with in their relative situation respecting the surrounding bones, affording a satisfactory proof that we are acquainted with all the parts of which the sternum is composed. In this specimen, the ribs can be traced to a greater extent than in any hitherto examined: they are not joined to the sternum by cartilages, but, as in the camelion and crocodile, are composed wholly of bone; and what is peculiar to themselves, each rib consists of one piece, having no intermediate joint: it describes a considerable curve in coming forward, and the outer side of the rib at that part is broader and stronger than any other. Their great length gives considerable depth to the chest.

In this specimen the bones are very small, but as they are completely formed, we must consider them to belong to a full grown animal.

When the vertebræ of the middle of the back in this specimen, are compared with those of the largest size that have been met with, it would appear that different species of the animal, were of very different sizes. In this specimen the diameter of a dorsal vertebra is only $\frac{5}{8}$ of an inch; in the largest that has been preserved it is 9 inches.

The drawing of this specimen is made by Mr. DE LA BECHE.

A specimen belonging to Colonel BIRCH, which in compliance with the wishes of my friend Mr. DE LA BECHE has been brought under my observation, contains nearly the entire skeleton of this extraordinary animal, and shows the important fact, that it had posterior as well as anterior feet; as it gives a posterior view, the bones forming the pelvis cannot be

made out, but these may be said to be the only ones with which we are now not acquainted.

This magnificent specimen is represented of its full size, in the annexed drawing, which has been made with so much ability by Mr. CLIFT, as to require no verbal explanation.

Since this Paper was laid before the Society, the author has received from Sir JOSEPH BANKS a specimen containing five fossil vertebræ of the Proteo-Saurus, each of them two inches in diameter, and one inch in thickness, found in the blue lias at Weston, near Bath, sent to him by the Rev. DANIEL LYSONS. This fact is important, as it proves that the fossil bones met with in the blue lias in that neighbourhood, which have always been considered to be those of the Crocodile, belong to this animal.

XIV. *Reasons for giving the name Proteo-Saurus to the fossil skeleton which has been described.* By Sir EVERARD HOME, Bart. V. P. R. S.

Read April 1, 1819.

IN the three Papers which I laid before the Society upon the subject of this fossil skeleton, I never ventured to hazard a conjecture upon the place in the chain of created beings, to which the animal belonged.

There were many circumstances which proved it to be unlike any animal at present in existence; some again making it an approach to the bird; others that connected it with fishes; so that I determined to prosecute the investigation till I had attained more satisfactory information respecting the skeleton, before I attempted to give the animal a name. This I think I have now done, the bones of the pelvis being the only ones not yet brought to light, and these are not necessary to enable us to make out the peculiar characteristics of the skeleton.

The discovery of the animal having four feet, established by the annexed drawings, removed it almost entirely from the finny tribe, in which there is no instance of such a mode of progressive motion.

It appears also distinct from the lacertæ, in which there is no instance of cupped vertebræ. All that tribe, as well as snakes and frogs, have the vertebræ united by regularly formed ball and socket joints.

These facts made it evident that the skeleton belonged to

an animal, somewhere intermediate between fishes and lizards, although belonging to neither; and the name *Ichthyo-saurus* has been suggested by those who saw it in that point of view.

Finding the farther I advanced in my investigation, that the approaches to the lizard were greater and greater, and the only association with fishes was in the cupped vertebræ, I was led to examine the vertebræ of the *Proteus*, three specimens of which Sir HUMPHRY DAVY had just sent me from Germany, and found them all deeply cupped at both extremities, and the intervertebral cavity filled with a fluid. I found the same structure in the vertebræ of the *syren* from Carolina, and in those of the *axolotl* from Mexico, Dr. LEACH having placed at my disposal a specimen of the *axolotl*, in all respects similar to that brought to Europe by HUMBOLDT, and so well described by CUVIER. In both of these last species the cavity was filled with elastic ligament, and in the *axolotl*, the septum between the two cups was not completely closed.

Mons. CUVIER, who has proved in so satisfactory a manner that the *proteus* and *syren* are completely formed animals, has expressed his doubts respecting the *axolotl*; and hints at its resembling the larva of the salamander; but leaves the matter open for future enquiry.

When it is mentioned that the salamander has ball and socket joints to its vertebræ, and those of the *axolotl* are cupped, that celebrated anatomist will agree that these animals belong to different genera; and admit that, if the *axolotl* is a larva, the complete animal must have cupped vertebræ, which structure, I believe, is only met with in the *proteus*,

the syren, and the axolotl; and these three when compared together, appear to be equally complete animals.

This opinion is strengthened by the observation, that the parts of the rana paradoxa which are removed when it becomes a frog, contain no bone, all the tail beyond the pelvis being soft cartilage. The same remark holds good with respect to the larva of the salamander, and I should believe with all other larvæ.

From this statement it appears, that the proteus from Germany, the syren from Carolina, and the axolotl from Mexico, not only agree in having lungs and gills, and therefore capable of breathing both in air and water; but in having feet, and cupped vertebræ, and therefore capable of employing both the mode of progressive motion of land animals and of fishes; and whatever variations there may be among themselves, yet as they all possess these two great distinguishing characters, which no other animals have, they must be allowed to form a distinct tribe, or more properly a distinct class, which, not to multiply terms, I shall call Proteus, till a more appropriate name is given.

The fossil skeleton resembles the Proteus tribe in having feet and cupped vertebræ, but differs from it in having long ribs attached to a regularly formed sternum, admitting of the chest being very capacious, and also in having no arches fitted for gills; it cannot therefore be called a Proteus, although allied to it, in having two modes of progressive motion. It resembles the lacerta in its mode of breathing, but differs from it in the mode of setting on the ribs on the spine, the form of the legs and feet, and the bony plates of the eye balls; it cannot therefore be called a lizard.

Its place in the chain of animal creation is clearly pointed out to be between the proteus and lizard, and will be sufficiently marked out by calling it Proteo-saurus.

EXPLANATION OF THE PLATES.

PLATE XIII. The representation of a portion of the skull of the Proteo-saurus, half the natural size, showing the form of the nasal bones immediately before the orbit. This is the only specimen in which these bones have been met with entire.

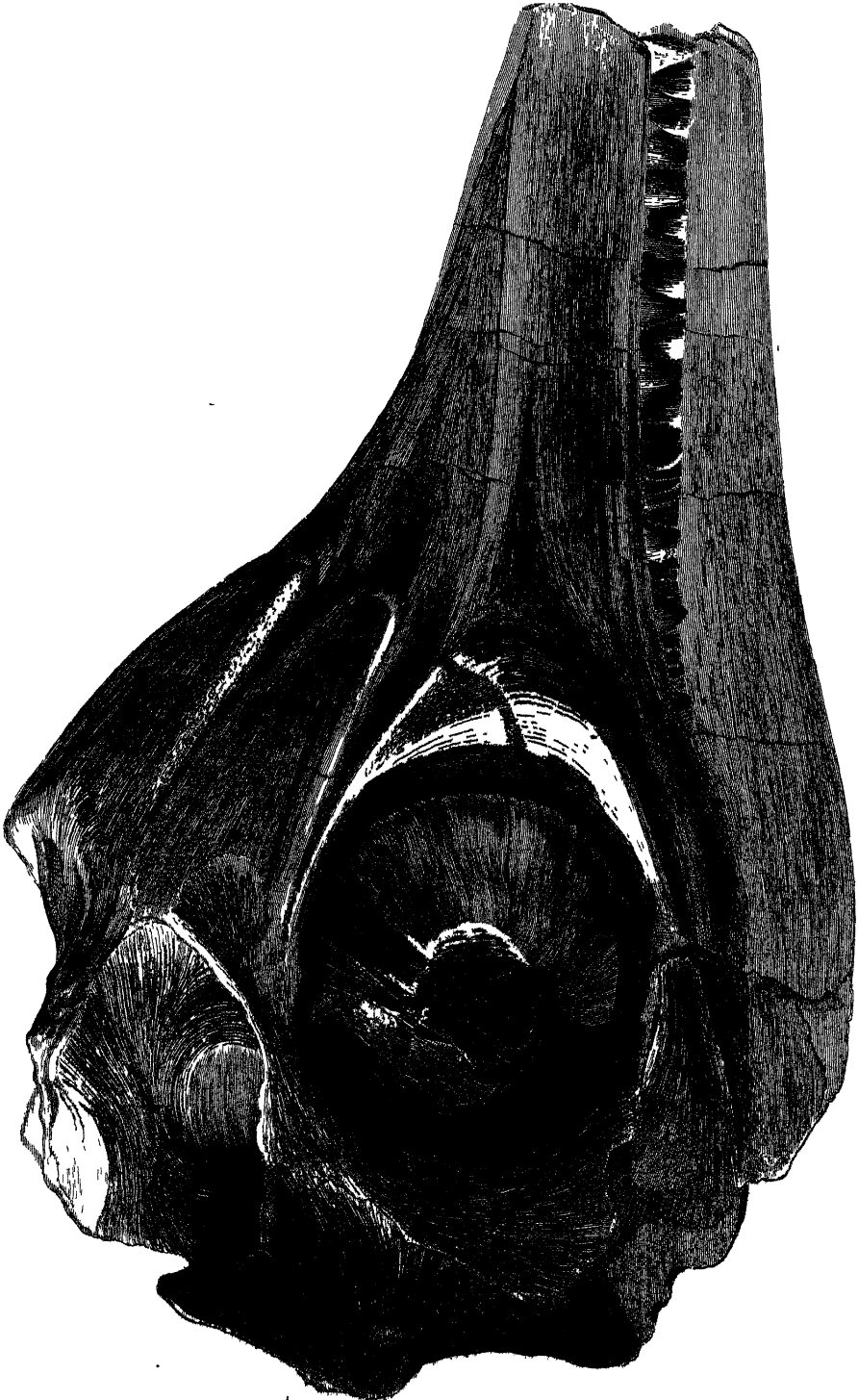
PLATE XIV. The representation of the sternum in an entire state, in its natural situation, confirming every thing shown in a Plate in a former paper, and determining its extent, which was not before so exactly known. The appearance of the ribs, shows that they come forwards towards the sternum in a bony form, as in the camelion, from which however they differ in having no joint, each rib being made up of one piece through its whole length, and at that part which forms the curve there is an increase of substance, making it stronger than the rest. There is something similar to this in the ribs of the chætodon of Sumatra, a description of which, by Mr. BELL, has a place in the 8^{grd} volume of the Philosophical Transactions. The figure is of the natural size, which is the smallest that has come under my observation; the drawing is made by Mr. DE LA BECHE.

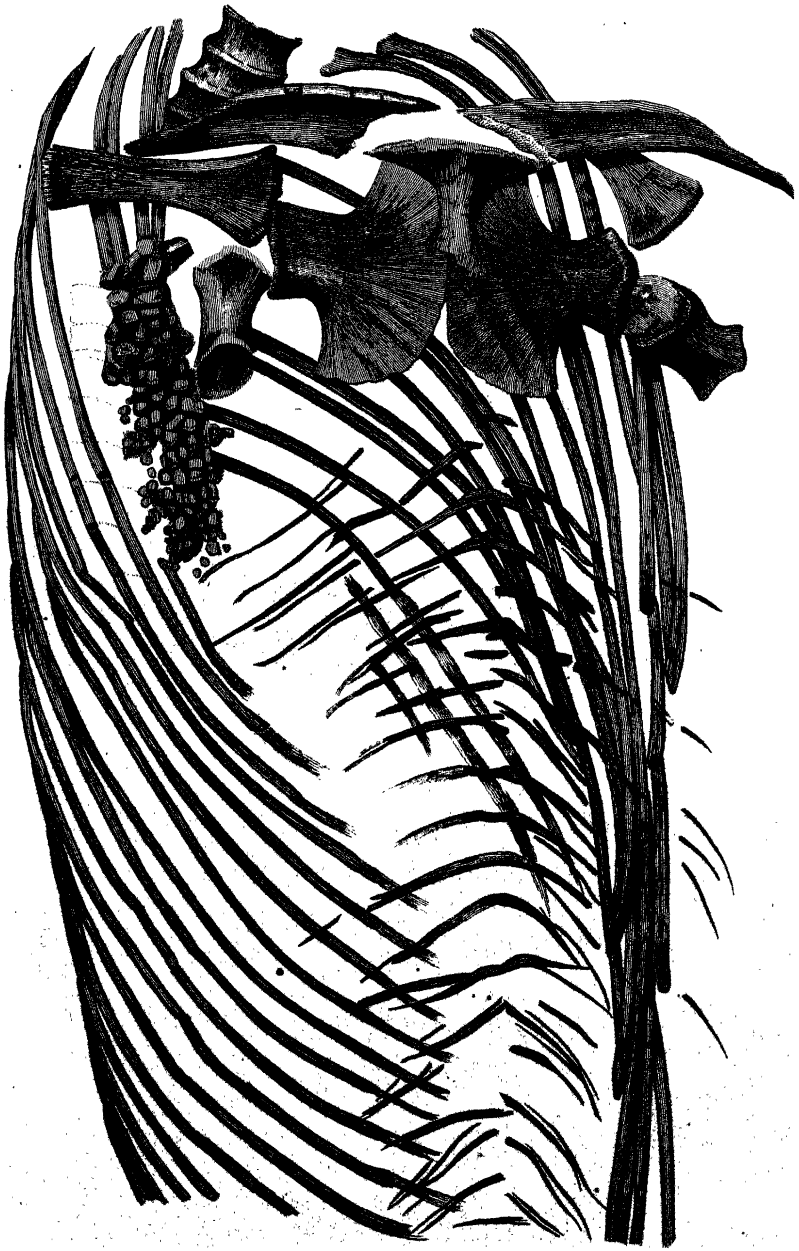
PLATE XV. Fig. 1. The representation of the skeleton of the Proteo-saurus, more entire than any hitherto met with; it is of the natural size. The different bones of which it is composed are sufficiently perfect, and sufficiently in their

places, to make any verbal explanation unnecessary. The drawing is made by Mr. CLIFT.

Fig. 2. A vertebra of the proteus from Germany, represented by Mr. BAUER, magnified ten times, to show the cup-formed extremities.

Fig. 3. A vertebra of the proteus from South Carolina, magnified four times, by Mr. BAUER, to show the same part.





XV. *Some Observations on the peculiarity of the Tides between Fairleigh and the North Foreland; with an explanation of the supposed meeting of the Tides near Dungeness.* By James Anderson, Captain in the Royal Navy. Communicated by the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S.

Read March 25, 1819.

HAVING observed that several Charts and Books of Navigation assert, that the tides from the North Sea and the Channel, or the Eastern and Western tides, meet in the vicinity of Dungeness and Rye harbour; and that, on such authority, this opinion has been too generally adopted by those, who have not had the opportunity or the inclination of making personal observations; as well as by the pilots on this part of the coast, who from being incapable, for the most part, of making observations or deducing inferences from facts before them, readily embrace the first theory they meet with *in print*, however erroneous or inconsistent; I have been induced to bestow all the attention in my power to the phænomena of the tides between Fairleigh and the North Foreland, and now venture to submit the result of my observations to the notice of the Royal Society. From having cruised constantly within these limits for nearly two years and a half, I have had many opportunities of making observations; but I must, nevertheless, profess my readiness to admit any alteration or improvement which may be pointed out by those more conversant with the subject; truth alone being the object of my enquiries.

I. *Phænomena of the Tides between Fairleigh and the North Foreland on the English coast, and Cape D'Alprèe and Calais on the French coast.**

The tides rise between the easternmost point of Fairleigh and the North Foreland from seven to eight feet higher than on either side of these points ; and during the last three hours and a quarter in which the tides run to the eastward, the water *falls by the shore*, making it half tide of ebb on the shore, or by the ground, when the current of the tide changes and begins to run from the eastward to the westward ; and it still continues to fall by the shore for two hours and three quarters after the tide has so changed ; at which time it is *low water* every where within these limits. The course of the tide continues to run to the westward two hours and three quarters longer, during which time the water gradually rises by the shore, making nearly *half flood* by the land, at the time the current of the tide ceases to run to the westward ; and returns again to the eastward, and continues to rise for three hours and a quarter, when it is high water by the ground. It then begins to fall again during the last three hours and a quarter, whilst the current of the tide sets to the eastward, as above stated ; and so on in continual rotation.†

These appearances have, no doubt, given rise to the

* In the detail I shall principally confine myself to the English coast, as the phænomena, and their causes and effects, on the French coast, within the same limits, are precisely similar.

† Every one who has attended to the tides, in general, knows that, where there are no local obstacles to prevent it, they flow regularly about six hours in one direction and then make high water, and ebb about the same time in a contrary direction, then making low water.

erroneous opinion that the tides meet between Dungeness Point and Rye Harbour ; but the real cause of these states of the tides within the particular limits I have described, seems to me to be the very great and sudden contraction of the channel between Dungeness and Cape D'Alpr  e, and the South Foreland and Calais Point. In that part it becomes, all at once, narrower by more than half of its width to the eastward or westward of these points. Dungeness is a long narrow point which projects from Winchelsea on the west side, and Hythe on the east side, to the extent of nine or ten miles, at least, directly into the sea across the channel ; and forms two deep bays, one on each side. Opposite to Dungeness is Cape D'Alpr  e on the French coast, jutting out also into the sea, so as to contract the distance between it and the opposite point to about twenty-four miles, and this cape has also a large bay on each side, of which Boulogne bay is to the eastward. (See the charts.)

The distance from the South Foreland to Calais is only eighteen or nineteen miles, and between these opposite shores lie the Ridge, the Varn, the Goodwin, and several other shoals on both sides of the channel, all of which serve to contract this strait still more. The western tide, therefore, coming up the English channel, meets with a resistance to its course at Dungeness and Cape D'Alpr  e, by the very sudden contraction of the space between these points ; where, from the passage being insufficient to discharge the quantity of water brought from the westward, it must necessarily accumulate, until it encreases the channel both by deepening and widening it, so as to become adequate to the discharge of the body of water supplied by the impulse of the tide.

This accumulation is of course the same every where within the straits of Dover (from Dungeness to a ridge of rocks to the eastward of the South Foreland), and also extends some distance without them, as far as the easternmost point of Fairleigh on the one side, and the North Foreland on the other; Dungeness west bay, and the Downs, forming as it were two large natural basons or reservoirs at each extremity of these straits, for the reception of the accumulated water, until it can find a passage. On this account the water must rise accordingly by the ground, or on the shore, during the time of this accumulation, wherever it takes place; and it is indeed found to be at its greatest height or making *high water by the ground*, at about three hours and a quarter after the tide of flood has run from the westward. At this time all the sands without the North Foreland are covered, and afford a greater vent for the discharge of the accumulated water. The extensive flats also on both sides of the channel, on which the sea now flows in like a torrent, demand a greater supply than is received through the Dungeness passage. From this period then (*viz.* three hours and a quarter after the regular flood tide has run to the eastward), the water is drawn off from the places where it had accumulated, and begins to fall gradually by the shore during the remaining three hours and a quarter, in which the current of the tide runs to the eastward; making *half tide of ebb* by the ground, within the straits of Dover and the two reservoirs or basons, when the current of the tide ceases to run to the eastward; at which time it is *high water* every where without these limits, allowing for the inequalities of the coast, the water having now generally acquired a level.

When it is high water without the North Foreland, as at Margate, the Kentish Knock, &c., and the tide, which is the true or regular ebb tide, returns to the westward through the Downs, the water still continues to fall within the Foreland, and on to the easternmost point of Fairleigh, for two hours and three quarters of the first of the true or regular ebb tide; because the tide is falling generally, and the passage by Dungeness discharges the quantity brought by the ebb tide during that time. But when the true or regular ebb tide has run two hours and three quarters, it is *low water* by the shore, between the North Foreland and Fairleigh; because the channel through the straits of Dover, (becoming again too contracted to give vent to the great body of water which now presses forward from the Medway and the North Sea, augmented by the currents and tides discharged from the great continental rivers and inlets,) now again accumulates in the narrow passage, and in the Downs, from the North Foreland, and thus begins, from the above stated period, to rise by the shore.

It thus continues to rise for the remaining two hours and three quarters, at which time the true or regular ebb tide has ceased to run to the westward, and it is *low water* every where without the North Foreland, and to the westward of Fairleigh. But within these limits (viz. between the North Foreland and Fairleigh), it is *half flood*, in consequence of the accumulation of the water during the latter part of the ebb tide. The true or regular ebb tide, or tide to the westward, now ceases to run, and the true or regular flood tide from the westward returns, bringing with it a greater quantity or body of water than the Dungeness passage can yet admit,

consequently it must accumulate in Dungeness west bay, and rises proportionally from thence along the shore to the North Foreland for three hours and a quarter of the first of the regular flood tide, because the tide is rising generally every where; and about this time the channel, becoming broader and deeper by the accumulation of the water and rising of the tide, is again sufficiently large to discharge the supply. The accumulated water being thus drawn off, as before mentioned, and with an accelerated current, to cover the flats and fill up the Medway, and the continental rivers, again begins to subside by the shore, at which precise period it is *high water* by the ground within the limits of accumulation, both on the English and French coasts; but without these limits it is only *half tide of flood*; and therefore the true or regular flood tide must run three hours and a quarter longer to the eastward, during which time the water falls by the shore, within the limits of accumulation, until it finds its level every where; and so rises and falls in perpetual rotation.

The tide, within the limits where the water accumulates, is found to rise from 28 feet to 30 feet perpendicularly, which is from 7 to 8 feet higher than it generally rises in the Channel. The following seem to be causes of this extraordinary rise. At half tide by the shore, within these limits, the water has found its level every where, and half the rise of the tide here, at high water (*viz.* 14 or 15 feet), being drained off to make high water without the North Foreland, and produce the level, is now contained in a space twice the breadth it formerly occupied; of course it follows, the same quantity of water will only be half the depth (or from 7 to 8 feet) that

it was when confined in half the space it now occupies. It may hence be inferred that the rise of the tide here, more than elsewhere, is nearly equal to about one quarter the rise of tide, whatever it may be; but as this must always depend upon local circumstances, as the same effects could not be produced if the situation was different, no general reasoning can apply. It has also been ascertained, that the true or regular flood tide runs six hours and an half to the eastward, while the true or regular ebb runs only five hours and an half to the westward; which makes the current of the tide run an hour longer to the eastward than the westward; but I have always found, from actual observation, that these tides are very much influenced by the winds.

Upon the whole, however, from the easternmost point of Fairleigh to the North Foreland, on both sides of the channel, it is always *high water by the ground*, when the true or regular flood tide has run three hours and a quarter from the westward; always *half ebb by the ground*, when the true or regular flood tide ceases to run from the westward; always *low water*, when the true or regular ebb tide has run two hours and three quarters from the eastward; always *half flood tide by the ground*, when the true or regular ebb tide ceases to run from the eastward; and always *high water by the ground again*, when the true or regular flood tide has run three hours and a quarter from the westward, or nearly so, and so on continually.

II. *Meeting of the Tides near Dungeness.*

Although the foregoing observations may not decidedly prove that *the meeting of the tides* cannot take place at or near

Dungeness, yet I trust, that they rationally and intelligibly account for the peculiar phænomena of the tides which occur there, without attributing them to the *meeting* of the tides, which could never produce these appearances. But if the tides ~~do~~ meet at Dungeness, they must meet in a line directly across the channel; for it is a fact so well established, that no one I believe has ever ventured to contradict it, that the western or true flood tide makes high water at Beachy Head, Fairleigh, Dungeness, and Deal, at nearly the same time as at Dieppe, the Soame, Boulogne, and Calais, the opposite points on the French coast, each to each; and that the eastern or regular ebb tide makes low water, at the same time, at the same places.

But if the tides met in a line across the channel, it must be evident to every one who has been at sea, that such a meeting would occasion so tumultuous a war of elements, between two large bodies of water impelled against one another, by the current of the tide and force of the winds, at a velocity of from four to six miles an hour, according to the age of the moon and strength of the wind, as would produce, from their furious and violent concussion, so great a sea, that no ship could venture to encounter it without the most imminent danger.

That this is not the case, experience daily proves; and therefore the absurdity and fallacy of the doctrine which asserts it, are obvious. But every master of a collier or coasting vessel, trading from the northward to any western port, as Portsmouth, Plymouth, &c., knows that the flood tide sets from the northward and eastward, along the English coast until he gets as far as the sand called the Kentish Knock:

and if he can reach it by high water, he calculates rightly, that he will have an ebb tide thence, which will carry him to the westward for nearly six hours longer. From this it is evident that the tides from the northward and eastward, and southward and westward, both meet at the Kentish Knock,* as they both make high water about the same time at the same point; and then the ebb tide recedes from this point in the opposite directions to which the flood had advanced. The formation of the coast too, by gradually altering the course of the flood tide between the South Foreland and Buoy of the Nore, from E. N. E. to W. N. W. within the stream of the Goodwin Sands (while without this sand it continues to run E. N. E. and easterly), in a great measure prepares for their meeting, without that wild commotion and furious contention which their coming together in a directly opposite line across the channel, would inevitably occasion. It also admits of their gently blending their waters together, and smoothly taking the same course, along both sides of the Long Sand, &c. the one, viz. the flood tide from the eastward up the King's Channel into the Thames, and the other (the flood tide from the westward through the Downs) up the Queen's Channel into the Medway, making only a strong eddy or whirlpool about the Knock, and a foamy rippling where they meet, as they proceed onwards together.

But, although the tide from the northward and eastward makes flood tide along the N. E. coast of England to the Kentish Knock, yet it is equally well ascertained, that the tide from the southward and westward makes flood tide along the opposite coasts of Flanders, Holland and Jutland,

* This sand is of a circular shape, so formed by the continually whirling eddy of the tides.

as far as the entrance of the Sleeve. From this last mentioned fact it evidently appears, that the flood tide from the westward forms two distinct branches at the Kentish Knock, taking different directions; the smaller of which, consisting of the stream of the tide *within* the Goodwin Sands, takes its course W. N. W. up the Queen's Channel, as before stated; whilst the larger, consisting of the stream of tide *without* the Goodwin, continues its course E. N. E. and easterly along the Flemish and Dutch coasts, until it is lost near the entrance of the Sleeve, in the great body of tide from the northward and eastward.

The opposite tides which meet in the North Sea do not meet in a line directly across any part of it, but in a *diagonal line*, extending from the Kentish Knock to the entrance of the Sleeve; where there is no tide, but a strong current, which almost always sets from the Jutland to the Norway side in the Sleeve; and which most probably proceeds from the eddy, produced by the great body of water coming round the Naze of Norway, meeting the remains of the western tide, aided by the reaction of the Jutland shore. In fact, there is hardly any tide observable between the Horne reef and the entrance of the Sleeve.

The tides thus meeting in a diagonal line in the North Sea, gently and gradually blend their waters together, without causing the least tumultuous appearance, exhibiting merely a little foamy rippling, which can be discerned in fine weather only, when the general mass of water is perfectly smooth.

To prove farther that this is the nature of the tides in this part of the North Sea, let a ship, for instance, sail from North Yarmouth, or Harwich, for the Texel or Flushing on

the opposite coast, with the wind from the north-eastward, so that she can lay her course on the *larboard tack*; the pilot will prefer getting under weigh at *high water*, on the English coast, to take the ebb tide under his lee; and if he can get half channel over during the ebb tide, or by the time of low water on the English side, he will find a flood tide from thence, setting along the opposite coast, which will also set under his lee for six hours longer, running in the same direction as the ebb tide did on the English side of the channel; and thus he will carry twelve hours tide with him; whereas had he continued on either side, he would have had a regular six hours tide each way; with this difference, that he would always have the tide setting in opposite directions on the one side, to what it would be on the other. That is; if the flood tide was setting to the *westward* on the English side, the flood tide would, at the same time set to the *eastward* on the Dutch side. Hence were a ship to sail, as above stated, with the wind so that she could lay her course on the *starboard tack*, she ought to get under weigh at *low water* on the English side, by which she would be able to carry twelve hours tide again under her lee, supposing her to reach the meeting of the tides at high water.

Every person who has been at Spithead may have observed, that the water rises there, and every where *within* the Isle of Wight, as far as Hurst Castle, for more than three hours after it is high water at the Owers, Dunnose, and every where *without* the Wight, and when the ebb tide has, of course, made to the westward; and that it is not high water at Spithead, Portsmouth Harbour, Southampton River, or any where within the Wight, until the ebb tide has run that time.

This is evidently occasioned by the narrow passage between Hurst Castle and the Island not having sufficient capacity to discharge the quantity of water brought by the ebb tide from the eastward through St. Helens; which therefore meeting with a resistance at Hurst Castle, accumulates and *rises* within the Wight, at the same time filling up Portsmouth Harbour, Southampton River, &c. &c. when the tide is falling every where in the English channel.

This circumstance arises from the same cause which occasions the tides to rise and fall in the Straits of Dover; with this difference, that it is high water by the ground, at the last mentioned place when the *flood tide* has run three hours and a quarter from the westward; but it is high water by the ground at the former, when the *ebb tide* has run about the same time from the eastward. It might therefore as well be asserted that the tides meet at St. Helens, Portsmouth Harbour, or Hurst Castle, as at Dungeness; but the fact is, that the phænomena which appear at these different places, are produced by the same cause producing similar effects, with only the difference occasioned by local circumstances in the time and manner; and this cause is the accumulation of the water brought forward by the tide; an accumulation which is occasioned by there not being a sufficient space for its discharge, in consequence of the contraction of the channel at the particular places where these phænomena are exhibited.

There is in fact a meeting of the tides, on a small scale, within the Wight; for the tide of ebb from Southampton River meets the tide of flood from the Needles, at the sand called Bramble (which has probably been originally formed by their meeting); from this they flow to Spithead, and meet

the tide of ebb from Portsmouth Harbour, at the sand called the Spit (perhaps also formed originally by their meeting there), and causing an eddy tide, which would deposit such sand, mud, &c. &c. as the current of the tide brings along with it: nor do I think it at all improbable that the Long Sand, at which I have stated the meeting of the tides through the Downs and from the North Sea to take place, has been likewise formed by the deposit of such things as the opposing tides brought with them, to the place where they met.

Being employed on the expedition against Walcheren, the laborious and difficult duty of passing the transports through the Slough passage into the West Scheldt devolved upon me, and afforded me an opportunity of observing another peculiarity of the tides in that place.

The Slough passage lies between Walcheren and South Beveland, communicating with the West Scheldt and the Veer Gat. From its junction with the last channel, the tide flows through several different channels between the islands, to the northward of South Beveland. On each side of the channels in the Veer Gat and Slough passage are extensive flats or mud banks, which begin to be covered about half tide of flood, and again begin to be dry about half tide of ebb. The flood flows regularly up the West Scheldt, carrying with it a vast body of water, which takes its course by Rammekins through the Slough passage, and meets the flood tide which flows up the Veer Gat at the north end of South Beveland; whence they flow together through the different channels formed by the adjacent islands. At high water the ebb sets again regularly down the West Scheldt and Veer Gat, but the ebb tide in the Slough continues to run to the northward, the same course as the flood tide, and passes down

the Veer Gat until the flats and mud banks become dry ; at which time the current of tide in the Slough changes, and runs to the S. Westward into the West Scheldt by Rammekins ; thus making the current of tide run nine hours one way, and only three hours the other. This may be accounted for in the following manner: when it is high water in the Scheldt, and the tide of ebb sets down the river, it sets over the extensive flat between the north-west point of South Beveland and Rammekins into the Slough, until the flat becomes dry, which occasions the tide to continue the same course as before, although the water is falling. But when all the flats become dry, and the water is confined within the proper limits of the respective channels of the West Scheldt, the Veer Gat, and the Slough ; and the Veer Gat being then only about 30 yards or less in width, three or four times narrower than the Slough, the water through the Slough cannot any longer find a vent through the Veer Gat, and therefore seeks one by the West Scheldt, where there is sufficient space for it; and hence the tide in the Slough changes and runs out by Rammekins into the Scheldt.

It would scarcely have been important to mention this peculiarity, as it is confined to a very small space, and where vessels of any considerable burthen never perhaps passed before the above mentioned expedition, and never may again; had it not on this occasion presented one of the greatest obstacles,* next to the continual adverse gales, which the transports had to contend with, in getting into the West Scheldt; and which could not have been overcome, but by dint of the most laborious and persevering exertions; and also as it furnishes a

* This obstacle, I confidently believe, was never known to the Commander in Chief, down to this moment.

proof of what I have before advanced, "that local circumstances will always have an effect upon the tides, to which no general reasoning can apply, in all straits and insular situations." These circumstances, however, may readily be ascertained by observation and by observation *only*.

J. ANDERSON. CAPT. R.N.

36 Hans Place, 6th February, 1819.

I have annexed a table showing the gradual rising and falling of the tides in Boulogne Bay, from soundings* taken every half hour whilst laying at anchor there, and which I think will greatly tend to confirm what I have advanced, with respect both to the rise and setting of the tides in the Straits of Dover, with the times of high and low water, and of the change of the current of tide there; circumstances which, I have reason to fear, have not been hitherto sufficiently attended to; but which would prove of the utmost importance, especially on expeditions where much boat service must be had recourse to; and in disembarking troops at a particular point, or in making an attack upon vessels at anchor during the darkness of the night; when a want of the necessary knowledge of the tides, or as it has often been called "a mistake in reckoning them," might be productive of the most fatal consequences.†

* As these soundings were taken with a common lead and line, and by different hands, I cannot venture to say that they were taken very accurately; and there might also be some irregularities in the ground, which would occasion a difference; and besides, they were taken 6 or 7 miles from the shore, where the tides do not rise quite so high as on the shore, owing to the re-action of the ground.

† A mistake in calculating the tide at this very place is mentioned by Lord NELSON, as a reason why the boats sent in by him to attack the French flotilla in Boulogne Bay, in 1801, did not get up with the enemy till long after the appointed time.

REFERENCES TO THE TABLE.

The first column contains the month, and day of the month.

The second column contains the wind and weather.

The third column contains the time the soundings were taken :

And the fourth contains the soundings, and the time the ship *tended*, or turned round with the tide. T. E. signifies tended to the eastward or to the flood. T. W. signifies tended to the westward or to the ebb tide.

Soundings taken in Boulogne Bay, at anchor, on 31st July, 1st, 2d, 3d and 4th August, 1811, on Board His Majesty's Sloop Rinaldo, Capt. ANDERSON.

Month and Days.	Wind and Weather.	Time.		Soundings in fathoms.	Month and Days.	Wind and Weather.	Time.		Soundings in fathoms.	Month and Days.	Wind and Weather.	Time.		Soundings in fathoms.
		H.	M.				H.	M.				H.	M.	
July 31, 1811. P.M.	Tended to E ^d . Fresh Breezes. NE.b.N.	3	P.M.	13 $\frac{1}{2}$			8	—	17 $\frac{1}{2}$			A.M.	30	16 $\frac{1}{2}$
		3	30	13 $\frac{1}{2}$			8	30	17 $\frac{1}{2}$			1	—	T.Wd. 16
		4	—	14 $\frac{1}{2}$			9	—	17 $\frac{1}{2}$			1	30	15 $\frac{1}{2}$
		4	30	14 $\frac{1}{2}$			9	30	H.W. 17 $\frac{1}{2}$			2	—	15 $\frac{1}{2}$
		5	—	15 $\frac{1}{2}$			10	—	17 $\frac{1}{2}$			2	30	15
		5	30	16 $\frac{1}{2}$			10	30	17			3	—	14 $\frac{1}{2}$
		6	—	16 $\frac{1}{2}$			11	—	16 $\frac{1}{2}$			3	30	14 $\frac{1}{2}$
		6	30	17			11	30	16			4	—	L.W. 14 $\frac{1}{2}$
		7	—	H.W. 17 $\frac{1}{2}$			Midnight	—	15 $\frac{1}{2}$			4	30	14 $\frac{1}{2}$
		7	30	17			A.M.	30	T.Wd. 14 $\frac{1}{2}$			5	—	14 $\frac{1}{2}$
		8	—	17			1	—	14 $\frac{1}{2}$			5	30	14 $\frac{1}{2}$
		8	30	16 $\frac{1}{2}$			1	30	14			6	—	15
		9	—	16 $\frac{1}{2}$			2	—	14			6	30	15 $\frac{1}{2}$
		9	30	Wd.T. 16			2	30	14			7	—	16 $\frac{1}{2}$
August 1, 1811. A.M.	Fresh Breezes. NE.b.N.	10	—	15 $\frac{1}{2}$			3	—	L.W. 14		August 3, 1811. Light variable airs inclining to calm.	7	30	T.Ed. 17
		10	30	15 $\frac{1}{2}$			3	30	14			8	—	17 $\frac{1}{2}$
		11	—	15 $\frac{1}{2}$			4	—	14			8	30	17 $\frac{1}{2}$
		11	30	15			4	30	14 $\frac{1}{2}$			9	—	18
		Midnight	—	14 $\frac{1}{2}$			5	—	14 $\frac{1}{2}$			9	30	18
		A.M.	30	14 $\frac{1}{2}$			5	30	15			10	—	H.W. 18
		1	—	14			6	—	15 $\frac{1}{2}$			10	30	18
		1	30	L.W. 13 $\frac{1}{2}$			6	30	T.Ed. 16			11	—	18
		2	—	14			7	—	16 $\frac{1}{2}$			11	30	17 $\frac{1}{2}$
		2	30	14 $\frac{1}{2}$			7	30	17			Noon	—	17 $\frac{1}{2}$
		3	—	14 $\frac{1}{2}$			8	—	17 $\frac{1}{2}$			P.M.	30	17
		3	30	14 $\frac{1}{2}$			8	30	17 $\frac{1}{2}$			1	—	16 $\frac{1}{2}$
		4	—	15			9	—	17 $\frac{1}{2}$			1	30	15 $\frac{1}{2}$
		4	30	Ed.T. 15 $\frac{1}{2}$			9	30	18			2	—	T.Wd. 15
August 2, 1811.	Fresh Breezes. NE.b.N.	5	—	15 $\frac{1}{2}$		Calm.	10	—	H.W. 18			2	30	14 $\frac{1}{2}$
		5	30	16 $\frac{1}{2}$			10	30	18			3	—	14 $\frac{1}{2}$
		6	—	16 $\frac{1}{2}$			11	—	17 $\frac{1}{2}$			3	30	14 $\frac{1}{2}$
		6	30	17 $\frac{1}{2}$			11	30	17 $\frac{1}{2}$			4	—	14
		7	—	H.W. 17 $\frac{1}{2}$			Noon	—	16 $\frac{1}{2}$			4	30	14
		7	30	17 $\frac{1}{2}$			P.M.	30	16			5	—	L.W. 14
		8	—	17 $\frac{1}{2}$			1	—	T.Wd. 15 $\frac{1}{2}$			5	30	14
		8	30	17 $\frac{1}{2}$			1	30	14 $\frac{1}{2}$			6	—	14 $\frac{1}{2}$
		9	—	17 $\frac{1}{2}$			2	—	14			6	30	15
		9	30	17 $\frac{1}{2}$			2	30	14			7	—	15 $\frac{1}{2}$
		10	—	17			3	—	14			7	30	15 $\frac{1}{2}$
		10	30	Wd.T. 16 $\frac{1}{2}$			3	30	L.W. 14			8	—	T.Ed. 16
		11	—	15 $\frac{1}{2}$			4	—	14			8	30	16 $\frac{1}{2}$
		11	30	15 $\frac{1}{2}$			4	30	14			9	—	16 $\frac{1}{2}$
August 3, 1811.	Fresh Breezes. NE.b.N.	Noon	—	14 $\frac{1}{2}$			5	—	14 $\frac{1}{2}$			9	30	17
		P.M.	30	14 $\frac{1}{2}$			5	30	14 $\frac{1}{2}$			10	—	17 $\frac{1}{2}$
		1	—	14 $\frac{1}{2}$			6	—	15			10	30	H.W. 17 $\frac{1}{2}$
		1	30	14 $\frac{1}{2}$			6	30	T.Ed. 15 $\frac{1}{2}$			11	—	17 $\frac{1}{2}$
		2	—	14 $\frac{1}{2}$			7	—	16 $\frac{1}{2}$			11	30	17
		2	30	L.W. 14 $\frac{1}{2}$			7	30	17			Midnight	—	17
		3	—	14 $\frac{1}{2}$			8	—	17 $\frac{1}{2}$			A.M.	30	17
		3	30	14 $\frac{1}{2}$			8	30	17 $\frac{1}{2}$			1	—	17 $\frac{1}{2}$
		4	—	14 $\frac{1}{2}$			9	—	17 $\frac{1}{2}$			1	30	17
		4	30	14 $\frac{1}{2}$			9	30	17 $\frac{1}{2}$			2	—	16 $\frac{1}{2}$
		5	—	T.Ed. 15 $\frac{1}{2}$			10	—	18			2	30	15 $\frac{1}{2}$
		5	30	15 $\frac{1}{2}$			10	30	H.W. 18			3	—	T.Wd. 15
		6	—	15 $\frac{1}{2}$			11	—	18			3	30	14 $\frac{1}{2}$
		6	30	16 $\frac{1}{2}$			11	30	17 $\frac{1}{2}$			4	—	14 $\frac{1}{2}$
		7	—	17			Midnight	—	16 $\frac{1}{2}$			Weighted.	—	
August 4, 1811.	Moderate Breezes, W.	7	30	17 $\frac{1}{2}$										

Full moon, and the tides, not yet at their highest.

XVI. *On the Ova of the different tribes of Opossum and Ornithorhynchus.* By Sir Everard Home, Bart. V. P. R. S.

Read March 25, 1819.

Now it is determined that the ova of quadrupeds in general are formed in corpora lutea, and that in all such animals the ova become attached to the uterus, and by this means the foetus receives its support and increase; we are enabled to ascertain the modes of formation of the ova of the opossum tribes, which from the want of this previous knowledge have not been investigated with the smallest degree of success. This becomes the best apology that can be made for the failure of every former attempt.

In this enquiry it will be found, that the ova of all the animals of these tribes are not formed in the same manner, and that the differences met with, make two distinct links between quadrupeds in general and the ornithorhynchi; these again approach so nearly to the bird, as to complete the links of gradation between the human species and the feathered race, so far at least, as concerns their mode of generation.

The mode of formation of the ova in the kangaroo, constitutes the first link in this beautiful series.

In the kangaroo, Mr. BAUER has found the corpus luteum, similar to that in quadrupeds; it is represented in the annexed drawing. Indeed it is to Mr. BAUER's talents and microscopical observations that we are indebted for all our information upon this subject.

In the kangaroo, the ovum when expelled from the corpus luteum, passes along the Fallopian tube, and as there is a thickening and glandular structure surrounding the portion of the tube next to its termination in the uterus, it is there that the yelk, or something analogous to it, is probably secreted; the ovum with the newly acquired yelk, drops from a pendulous opening into the uterus, in which it receives the albumen. In one specimen the uterus in a pregnant state, came under my observation; but as it was sent from New South Wales, in spirit which had not been timely renewed, the contents of the uterus were reduced to a confused mass, in which only a part of the bones could be made out; enough was however seen, to determine that the ovum of the kangaroo in the uterus has an abundant supply of albumen. There was no attachment whatever between the albumen and the uterus. There are two lateral canals that communicate between the uterus and vagina: these answer the purpose of aerating the foetus by means of atmospheric air.

As the penis of the male has only one orifice at the point adapted to the os tincæ, the ovum must of necessity be impregnated in the uterus, the structure of the Fallopian tubes, and their mode of terminating in the uterus, rendering it impossible for the semen to pass into them.

The foetus, as soon as it arrives at a certain size (at which time it in general weighs about 12 grains,) is expelled from the uterus, and is received into the marsupium, where it becomes attached to the point of one of the nipples, at first by simple contact; but as the foetus grows, the nipple is found farther in the mouth upon the surface of the tongue. In the 85th volume of the Philosophical Transactions, Plate XVIII. XIX. XX. and XXI. and in the 100th volume, Plate XIII. most of

these circumstances are represented, but the uses of the different parts, I readily confess; I was at that time unable accurately to comprehend.

The mode of formation of the ova in the Koli and the Wombat of New South Wales, and in the great and small Opossum of North America, constitutes the second link in this chain of gradation.

In none of these different genera are there corpora lutea in the ovaria, but in their place a certain number of yelk-bags of different sizes; and these are so completely imbedded in the substance of the ovarium, that to common observation they appear to be so many corpora lutea. There is no thickened glandular structure surrounding the Fallopian tubes near their termination in the uterus. Instead of one uterus having two Fallopian tubes, there are two uteri and one tube to each; and in proof that the ovum in each uterus is impregnated separately in its own cavity, the point of the penis in the male is so formed as to throw the semen into both. The lateral tubes, by which the foetus is aerated in the kangaroo, are formed in these genera in a different manner; there is only one to each uterus, and this, instead of communicating with the uterus at the fundus, opens into it at the cervix. The yelk-bags are shown in the annexed drawing. I have not had an opportunity of examining the ovum of any of these animals in utero, but Mr. BELL, a very intelligent surgeon, transmitted an account to Sir JOSEPH BANKS, of the dissection of a female koli, in which he met with an imperfectly formed embryo in each of the uteri, surrounded by a mass of albumen. The young of all these genera are expelled from the uterus into the marsupium, and become attached to the prominent points of the nipples.

The mode of formation of the ova in the ornithorhynchi, constitutes the intermediate link between that of the American opossum and the bird.

The yelk-bags in the ovaria of the ornithorhynchi are more distinct, and less deeply imbedded than in the opossum : there is no regular uterus, nor Fallopian tubes ; the yelk-bags pass along an oviduct, the lower part of which performs the office of a uterus. In this situation the ova are impregnated ; the penis of the male, which is bifid, throwing the semen into both oviducts at the same time, through several points like a watering pot, so as to scatter it all over the cavity. The ova are aerated by the vagina. The ova in a magnified state are represented in the annexed drawing. The organs of generation are figured in the 92nd volume, Plate IV. of the Philosophical Transactions. To show that the yelk-bags in the ornithorhynchus resemble those of the pullet, a magnified drawing of them made by Mr. BAUER in that bird, is annexed.

To those members not conversant in comparative anatomy, the following summary may be acceptable.

In the human species, and quadrupeds in general, the ova are formed in corpora lutea, and pass into the uterus, to the sides of which they become attached ; when the foetus is completely formed it is expelled by the vagina, and afterwards sucks the mother.

In the kangaroo the ova are formed in corpora lutea, receive their yelks in the Fallopian tube, and their albumen in the uterus. The ovum thus completed, is impregnated in the uterus, aerated by means of lateral tubes, and when the young is expelled from the uterus, it is received into the marsupium, and attached to the nipple of the mother.

In the American opossum, the yelk bags are formed in the ovaria; pass into the uteri, there receive the albumen, and are then impregnated; the foetus in each uterus is aerated by one lateral tube. When expelled from these uteri, the young are received into the marsupium, and become attached to the nipples of the mother.

In the ornithorhynchi the yelk-bags are formed in the ovaria; received into the oviducts, in which they acquire the albumen, and are impregnated afterwards; the foetus is aerated by the vagina, and hatched in the oviduct, after which the young provides for itself, the mother not giving suck.

In the pullet, the yelk-bags are formed in one ovarium, impregnated in one oviduct, and hatched out of the body.

EXPLANATION OF THE PLATES.

PLATE XVI.

Ovarium of the Kangaroo.

Fig. 1. The ovarium of a young kangaroo laid open: natural size; showing the corpus luteum.

Fig. 2. The same section magnified four diameters, to show the corpus luteum more distinctly.

Fig. 3. A section of the corresponding ovarium of the same kangaroo, magnified four diameters; to show an incipient corpus luteum.

Fig. 4. A similar section of the ovarium of an old kangaroo: natural size; in which there are two corpora lutea.

Fig. 5. The same section, magnified four diameters.

PLATE XVII.

Ovarium of the American Opossum.

- Fig. 1. Front view of the ovarium of the large American opossum : natural size.
- Fig. 2. The same view, magnified five diameters.
- Fig. 3. Back view, magnified five diameters.
- Fig. 4. A perpendicular section, magnified five diameters.
- Fig. 5. The same section, magnified ten diameters.
- Fig. 6. A young yelk-bag, magnified twenty diameters.
- Fig. 7. A full grown yelk-bag, magnified twenty diameters.
- Fig. 8. A full grown yelk-bag opened, to show its contents ; magnified twenty diameters.

PLATE XVIII.

Ovarium of the Ornithorhynchus Paradoxus.

- Fig. 1. Front view of the ovarium of the ornithorhynchus paradoxus : natural size.
- Fig. 2. The same view, magnified five diameters.
- Fig. 3. Back view, magnified five diameters.
- Fig. 4. A small portion cut off from the upper end of the left side of Fig. 3. ; magnified ten diameters.
- Fig. 5. An internal view of the same portion, magnified ten diameters, to show the yelk-bags.
- Fig. 6. A full grown yelk-bag, magnified twenty diameters.
- Fig. 7. A young yelk-bag opened, to show its contents : magnified twenty diameters.
- Fig. 8. A full grown yelk-bag opened, to show its contents : magnified twenty diameters.

Fig. 9. The globules of the yelk diluted with water, magnified four hundred diameters.

PLATE XIX.

Ovarium of the Hen.

Fig. 1. Front view of the ovarium of a hen : natural size.

Fig. 2. A small portion of the same, with some very young yelks : natural size.

Fig. 3. The same small portion, magnified five diameters.

Fig. 4. Back view of the ovarium of the hen : natural size.

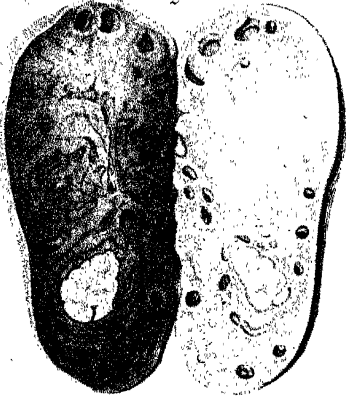
Since this Paper was sent to the press, the author has received, through the kindness of Governor MAC QUARRIE and Sir JOHN JEMISON, four specimens of female Ornithorhynchi Paradoxi from New South Wales, and finds in all of them, as well as in every other specimen that has come under his observation, that there are yelk-bags only in the left ovarium, showing that both ovaria are not generally in use at the same time. This is an approach to the condition in which there is only one, lying on the left side.

In the chick of the common fowl before it is hatched, there is a small portion of an ovum on the right side, but this disappears before the chick is completely formed.

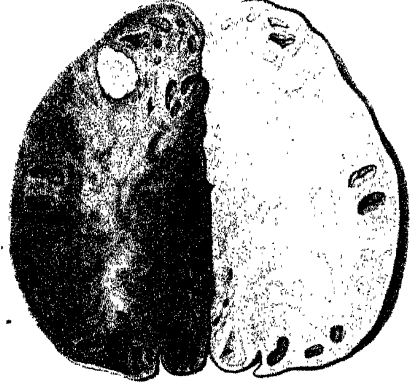
Fig 1



2



3



4



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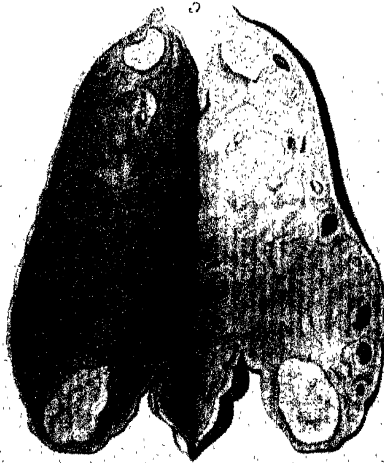
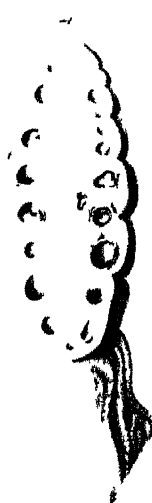




Fig 1



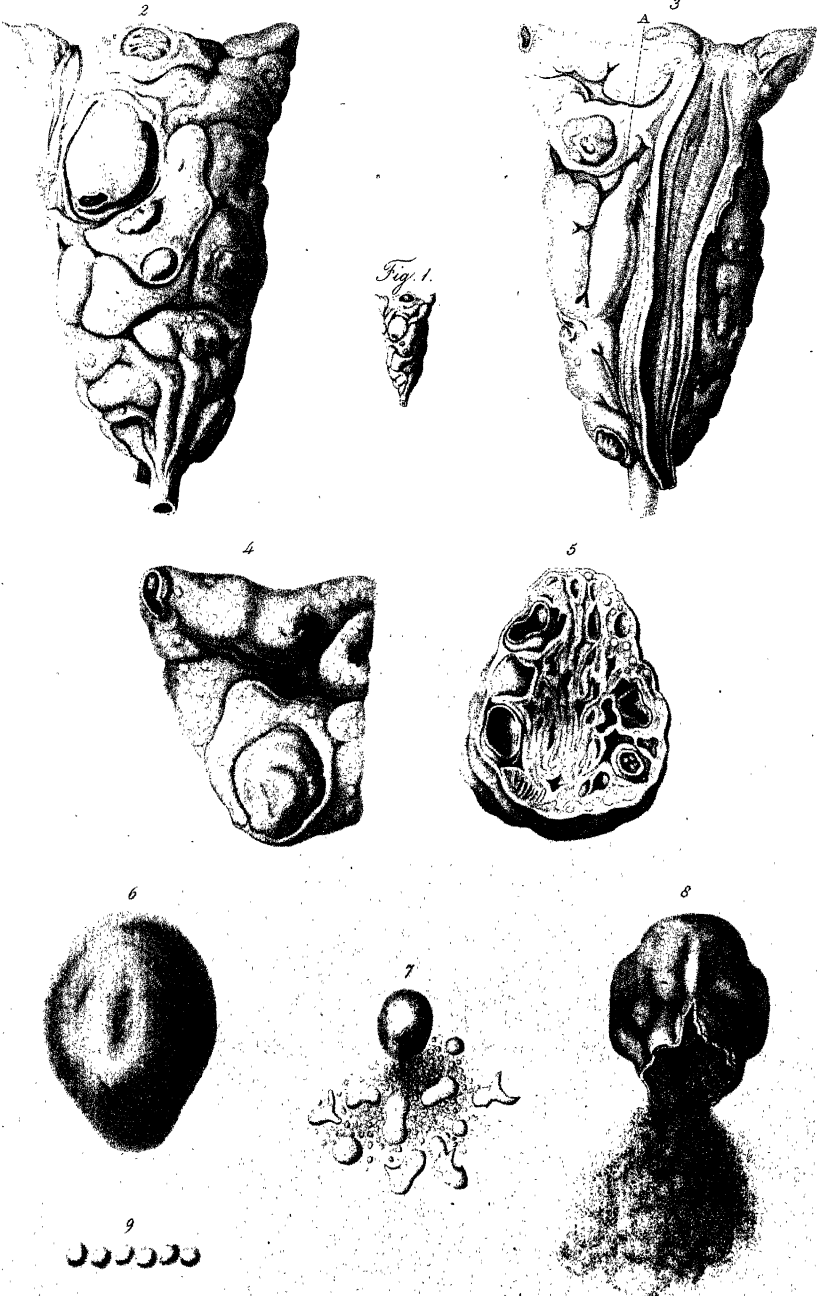
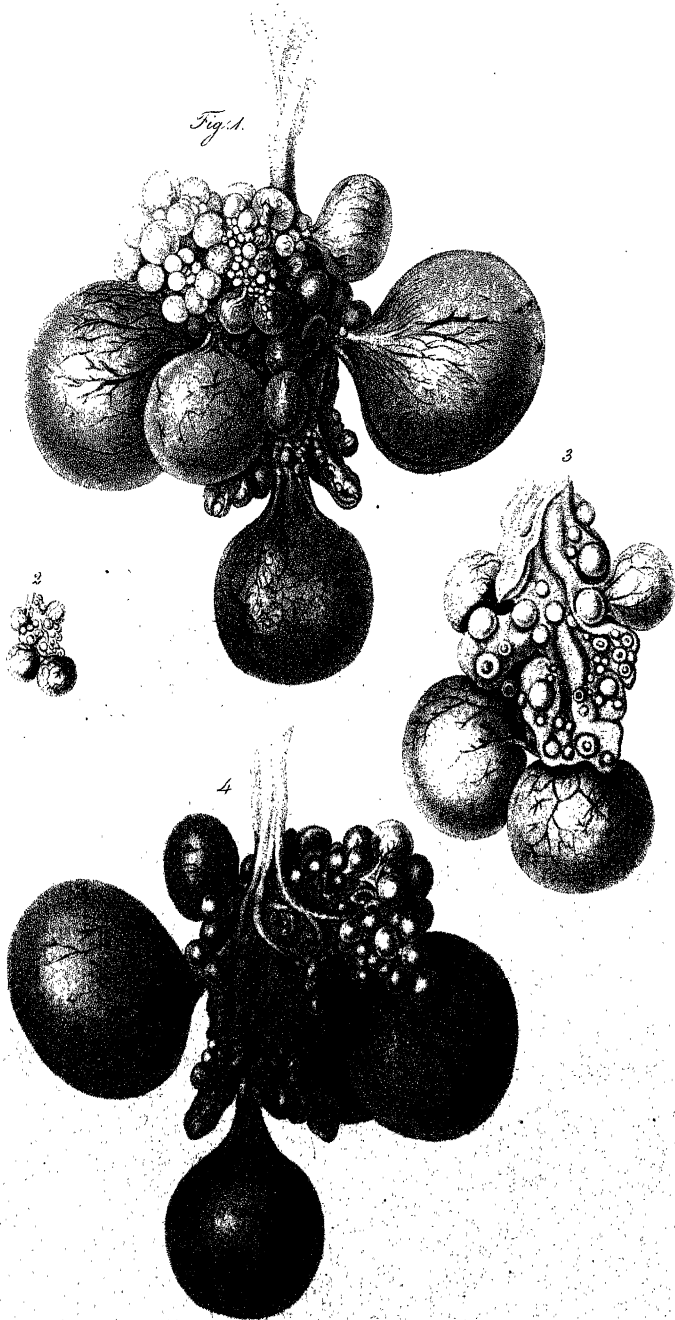


Fig. 1.



XVII. *The results of Observations made at the Observatory of Trinity College, Dublin, for determining the Obliquity of the Ecliptic, and the Maximum of the Aberration of Light. By the Rev. J. Brinkley, D. D. F.R.S. and M.R.I.A. and Andrew's Professor of Astronomy in the University of Dublin.*

Read April 1, 1819.

OBSERVATIONS have been made by the eight feet circle of the Observatory of Trinity College, Dublin, at the respective summer solstices since the year 1809, with the exception of two. The obliquity of the ecliptic thence resulting, has always agreed so nearly with that adopted in the French tables, that I have heretofore thought it useless to make any public communication relative thereto. But some circumstances have now induced me to lay my results before the Royal Society.

The recent publication of Mr. BESSEL's valuable labours on the observations of Dr. BRADLEY, has afforded us a more exact determination of the obliquity of the ecliptic, as deduced from the early observations by the Greenwich quadrant, than we before possessed. The comparison of this with the present obliquity, gives us the diminution for an interval of nearly 60 years, with a considerable degree of accuracy, and almost sufficient to enable us to place the confidence in the mass of results.

To obtain this result with a greater degree of certainty.

the present obliquity, as deduced from a mean of the observations of different astronomers, should be used.

It has been an opinion almost generally received among astronomers, that observations of the winter solstice, have given a less obliquity of the ecliptic than observations of the summer solstice.

The explanation of this seemed very difficult. But in the above mentioned work of Mr. BESSEL, he calls in question this opinion, and shows that the observations of Dr. BRADLEY give the same result, both in summer and winter. His own observations also tend to the same conclusion. The observations of Dr. MASKELYNE, of M. ORIANI, of M. ARAGO, and of Mr. POND, are in opposition to these ; to which my own may be added.

It is not likely that this difference really exists, but it is a question of some importance in astronomy, and the explanation thereof may throw some light on other points.

It is probable the difference arises from some unknown modification of refraction. I find, and I believe other observers have found the same, that at the winter solstice, an irregularity of refraction takes place for the sun greater than for the stars, at the same zenith distance. The zenith distance of the sun at this place is then nearly 77° .

What Mr. BESSEL has adduced, certainly tends to render the prevalent opinion doubtful. It therefore appears to me of consequence, that astronomers should pay attention to the observations at the winter solstice. My observations at that time have been much fewer than in the summer, because, on account of the uncertainty of refraction, I considered them of less importance.

It has been proposed to make the two results agree, by an increase of the quantity of BRADLEY's mean refraction; but this could not be done, without increasing it by a quantity greater than can be justified by other determinations respecting refraction.

Considering then this uncertainty respecting the observations of the winter solstice, it appears better to compare the results from Dr. BRADLEY's summer solstices, with the result as deduced from the mean of the observations of different astronomers.

Mean Obliquity, Jan. 1, 1813.

M. ORIANI*	4 summer solstices	23° 27' 50",34
Mr. POND†	2 summer solstices	23 27 50 ,37
Mr. ARAGO‡	2 summer solstices	23 27 50 ,09
Dr. BRINKLEY	8 summer solstices	23 27 50 ,99
Mean Jan. 1. 1813		23 27 50 ,45
Dr. BRADLEY, Jan. 1. 1755		23 28 15 ,49
diff. 58 years.		25 ,04

This gives 0",43, for the annual diminution.

The mean of 18 observations near the winter solstice gives the mean obliquity Jan. 1, 1813, 23° 27' 48",14.

The above determination of the obliquity by observations near the summer solstice gives (taking the annual diminution 0",43.)

Mean obliquity Jan. 1, 1800 = 23° 27' 56",0, differing only 1" from that assumed in M. DELAMBRE's tables of the sun.

* See Mr. BESSEL's work, p. 62.

† Phil. Trans. 1813, p. 304. This is corrected for the solar nutation.

‡ Conn. des Temps. 1816. The observations were made with a three feet repeating circle.

And as far as my own observations are concerned, the difference does not exceed half a second.

In M. ZACH's solar tables, there is given a determination of the obliquity of the ecliptic computed by M GERSTNER, from a mean of a great many observations of Dr. MASKELYNE's, made at 19 summer solstices. Although the results of the several solstices are rather discordant, more so than was to be expected from a fixed instrument, yet it is likely a mean of 173 observations cannot be far from the truth.

This mean is $23^{\circ} 28' 11'',0$ for 1769,

when reduced to 1800, is $23 \ 27 \ 57 \ 7,$

which agrees sufficiently near with the present determination, to show that, if the necessary corrections for the sun's latitude, &c. had been used, the result would probably have been very exact.

The mean of 102 observations at 17 winter solstices computed by M. GERSTNER, gives for 1769— $23^{\circ} 28' 3''$; a result which, after making all possible allowances for the error of the quadrant, is considerably less than that deduced from the summer solstices.

In using the eight feet circle, two or more observations were made a few minutes before the sun arrived at the meridian, and then the instrument was reversed, and observations made after the passage. The results were carefully reduced to the meridian; the upper and lower limbs being observed, the zenith distance of the centre was deduced from the instrument itself. This facility of reversing the instrument seems more likely to produce exact results, than those obtained by a fixed instrument, although from the necessary effect of the action of the sun on the parts of the instrument, the results must be expected to be more discordant than those obtained by a fixed instrument.

The results of the several observations are as follow.

Time of Observation.	Observed Declination.	Corr. for ☉'s Lat.	Mean Obliquity re- duced to Jan. 1, 1813.
1809. June 9	22 56 4,34	+ 0,24	23 27 51,43
14	23 16 24,84	+ 0,82	50,85
15	23 19 15,49	+ 0,85	50,76
17	23 23 41,83	+ 0,84	49,56
18	23 25 15,58	+ 0,82	46,67
19	23 26 28,74	+ 0,74	47,87
22	23 27 37,58	+ 0,32	49,58
27	23 21 17,24	- 0,40	52,76
1810. June 1	22 0 37,31	+ 0,49	23 27 50,00
6	22 37 23,04	+ 0,64	47,55
20	23 27 7,65	- 0,55	49,43
22	23 27 43,28	- 0,29	53,43
1811. June 18	23 24 35,59	+ 0,63	23 27 52,67
19	23 25 58,05	+ 0,68	51,07
22	23 27 40,65	+ 0,66	50,78
1813. June 22	23 27 41,28	+ 0,25	23 27 53,58
24	23 26 17,22	+ 0,34	50,07
25	23 24 59,75	+ 0,33	49,97
26	23 23 17,68	+ 0,28	50,06
28	23 18 38,88	+ 0,05	49,36
1814. June 15	23 18 40,32	+ 0,65	23 27 49,01
19	23 26 21,29	+ 0,07	51,22
21	23 27 40,26	- 0,26	49,63
22	23 27 42,88	- 0,42	49,23
23	23 27 21,44	- 0,56	49,63
24	23 26 35,25	- 0,65	50,02
25	23 25 23,89	- 0,69	50,12
1815. June 21	23 27 41,40	+ 0,03	23 27 52,78
22	23 27 48,76	+ 0,16	51,48
27	23 22 24,08	+ 0,73	54,56
28	23 20 1,40	+ 0,76	51,46
29	23 17 16,13	+ 0,76	50,45
1816. June 16	23 22 29,42	+ 1,15	23 27 52,31
21	23 27 50,73	+ 0,88	51,23
28	23 18 3,91	- 0,11	53,61
1818. June 11	23 4 50,08	- 0,76	23 27 49,23
12	23 9 2,50	- 0,64	53,29
18	23 25 20,22	+ 0,33	54,81
20	23 27 26,32	+ 0,67	53,23
22	23 27 55,72	+ 0,51	53,53
24	23 26 44,04	+ 0,35	51,92
30	23 13 20,25	- 0,42	51,53

In the paper which I had the honour of presenting to the Royal Society last year, I mentioned my doubts as to the quantity of the maximum of the aberration of light, and that, as far as could be ascertained from Dr. BRADLEY's Wanstead observations with a zenith sector, we ought rather to adopt $20''$,00 than $20''$,25. I also mentioned that it would be desirable to investigate this point, and therefore during the last year, I instituted a course of observations for this purpose, and I beg leave to offer the results thereof.

	No. Ob.	Max. Aber.	N. P. D. By Observations in 1818.	N.P.D. Before.
α Cassiopeæ	22	20,72	34 27 43,34	43,59
Polaris	23	20,73	1 39 44,55	44,27
α Ursæ Maj.	23	20,04	27 16 7,50	7,38
γ	27	21,20	35 17 34,83	36,22
ϵ	30	21,36	33 3 0,26	0,45
ζ	20	20,15	34 7 15,31	17,03
η	21	21,12	39 46 29,15	29 37
	166	20,80		

By these the maximum appears to be $20''$,80, which is much greater than I had expected. While these observations were going forward, Mr. BESSEL's work above mentioned was published. From several investigations in the Greenwich observations of Dr. BRADLEY, he also deduced the maximum = $20''$,70, nearly. These results certainly appear extraordinary, and are not likely to be acknowledged by astronomers, unless they shall be established by a great number of observations.

My results were computed with great care, allowances being made for the ellipticity of the earth's orbit. It is not likely, supposing the velocity of the light of all the stars

to be the same, that the result can err more than $\frac{1}{4}$ of a second.*

By continuing the observations, I hope to obtain farther information on this interesting point. And it appears to be an enquiry deserving of the joint co-operation of astronomers.

Those instruments which admit of observing each star, without a reference to other stars, seem best adapted thereto. It is not likely that the maximum of aberration differs in different stars; yet this ought not to be taken for granted.

The mean N.P.D. Jan. 1, 1818; deduced from former observations, have been put down as a proof of the consistency of my instrument. ζ Ursæ Majoris is the only star in which the difference is worth notice. Whether this difference is from the error of observation, or from any uncertainty in the proper motion of the star, it is difficult to say. Three results reduced by BRADLEY's refraction are as follow.

		N.P.D. Jan. 1, 1815.
My observation,	1812	$34^{\circ} 6' 19''.99$
Mr. POND's observation,	1815	18 ,92
My observation,	1818	17 ,67

A comparison of independent results is for many reasons much to be desired. I offer the above principally with a view of calling the attention of astronomers to such investigations.

* The observations of Mr. POND with the fixed telescope, may be adduced as contrary to my results; because with this maximum of aberration, his summer and winter differences of N. P. distance of β Aurigæ and α Cygni would differ by $1''$ in a direction contrary to parallax. But it also seems to show the necessity of exact determination of the precise quantities of the equations for N. P.D. before any conclusive arguments respecting the non-existence of parallax, from observations of the positions of stars relative to each other, can be adduced. In observations by the eight feet circle this is not so necessary, as has been before mentioned.

It appears to me, that the only method by which an explanation of the difficulties that have occurred, from a comparison of the Greenwich observations and of those made at this Observatory, can be obtained, is from an *extensive* series of observations of many stars, referring each to the apparent zenith point. I am therefore pursuing such a course of observations. Conclusions as to the existence or non-existence of parallax, from comparisons of the *relative* places of stars taken indiscriminately, must be liable to much uncertainty, whether the comparisons be made by polar distances or by right ascensions. The former being affected by the uncertainty of refraction, may, at first view, be thought more subject to error than the latter; but a careful consideration of the circumstances attending the latter method, will show that it has its peculiar difficulties.*

* As Mr. BESSEL's determination of the maximum of aberration has been referred to, it may also be right to mention his results respecting the parallax of certain stars. He uses transit observations of stars nearly opposite in right ascension (p. 110. &c.) Thus he finds the sum of the parallaxes of Sirius and α Lyræ insensible, and the sum of the semi-parallaxes of Procyon and α Aquilæ, nearly 1". This method of using the transit observations is undoubtedly far preferable to that of using them indiscriminately. With respect to the observations Mr. BESSEL had to compute from, I think it must be allowed they were not sufficiently exact, to give much weight to his conclusions. The methods of observing with the transit, and of entering the observations, were then far inferior to the present. This objection, however, does not apply to the observations of the pole star, and therefore does not affect the maximum of aberration deduced from the observed right ascension of that star.

Observatory, Trinity College, Dublin, February 13, 1819.

XVIII. *On some new Methods of investigating the Sums of several Classes of infinite Series.* By Charles Babbage, Esq.
A. M. F. R. S.

Read April 1, 1819.

THE processes which it is the object of this paper to explain, were discovered several years since ; but certain difficulties connected with the subject, which I was at that time unable to explain, and which were equally inexplicable to several of my friends, to whom I had communicated these methods, induced me to defer publishing them, until I could offer some satisfactory solution.

These observations refer more particularly to the second method which I have detailed in this paper, and which may not inappropriately be called the *method of expanding horizontally and summing vertically*. Some traces of this method may, perhaps, be found in former writers, and particularly in a paper by Professor Vince, "On the Summation of Series," printed in the Philosophical Transactions for 1791 ; but there exists this peculiarity in that which I have employed, that after a certain number of the vertical columns are summed, all the remainder either vanish, or else have some common factor. This method, which I employed about the year 1812, gave the values of a variety of series whose sums had not hitherto been known, most of which were apparently correct, but some of the consequences which followed were evidently erroneous. About this time, Mr. HERSCHEL, to whom I had

communicated these anomalous results, by following a very different course, arrived at several general theorems, which, when applied to the series I had obtained, gave the same results. This coincidence at first increased my confidence in the values so discovered, and I continued to examine the reason why my own formulæ were in some cases defective. Mr. HERSCHEL'S method was published in the Philosophical Transactions for 1814; and it was not until some time after that I perceived, that although the investigations were very different, the fundamental principle was the same in both methods. This induced me to attempt summing the same series by a direct process, and I succeeded in obtaining their sums by integration relative to finite differences, aided by certain peculiar artifices. The results obtained by this new plan, which is the first treated of in this paper, coincided with those already found, and seemed to confirm their truth, without in the least indicating the cause of the error: this cause however I now began to suspect, and, after some enquiry, I was at length able to detect. I have found that the *method of expanding horizontally and summing vertically*, will always lead to correct results, provided a certain series which I have pointed out, is finite. I have also shown how to express this series by a definite integral; and when this integral or this series has a finite value, the method may be depended on. In case this series or this definite integral is not finite, then the value of the series* multiplied by zero, must be added to

* The investigation of this series is generally a task of considerable difficulty. I have however given an example, wherein the correction thus found, added to the sum indicated by the method we are considering, gives the true value of the series, which in this case is one whose sum has been found by Euler.

the sum given by this method. In this latter case, however, the mode of summation which I have proposed, is not well adapted for giving the sums of series; its greatest advantage is felt when the integral or series alluded to is finite: but even in this case the criterion I have pointed out is not useless, for it serves to except certain particular values of the variables, which would give incorrect results. Without this criterion, or without something equivalent to it, I am inclined to think that the principle on which this method is founded, although it will probably in many cases give accurate results, will in others produce such as are not only numerically but symbolically untrue. It is worthy of remark, that the *method of expanding horizontally and summing vertically*, in many instances, gives precisely the same formulæ as the direct process of integration; yet that that method attaches limitations to them, which are necessary to their accuracy, but which are not indicated by the method last mentioned.

Before I proceed to explain these two processes, it will be convenient to prove that the values of all series of the forms

$$A_1 x \frac{(\sin \theta)^m}{(\cos \theta)^n} + A_2 x^2 \frac{(\sin 2\theta)^m}{(\cos 2\theta)^n} + A_3 x^3 \frac{(\sin 3\theta)^m}{(\cos 3\theta)^n} + \&c.$$

$$A_1 x \frac{(\cos \theta)^m}{(\sin \theta)^n} + A_2 x^2 \frac{(\cos 2\theta)^m}{(\sin 2\theta)^n} + A_3 x^3 \frac{(\cos 3\theta)^m}{(\sin 3\theta)^n} + \&c.$$

depend on series of the form

$$A_1 \frac{x}{(\cos \theta)^n} + A_2 \frac{x^2}{(\cos 2\theta)^n} + \&c.$$

$$A_1 x \frac{\sin \theta}{(\cos \theta)^n} + A_2 x^2 \frac{\sin 2\theta}{(\cos 2\theta)^n} + \&c.$$

and

$$A_1 \frac{x}{(\sin \theta)^n} + A_2 \frac{x^2}{(\sin 2\theta)^n} + \&c.$$

$$A_1 x \frac{\cos \theta}{(\sin \theta)^n} + A_2 x^2 \frac{\cos 2\theta}{(\sin 2\theta)^n} + \&c.$$

or else they depend partly on these and partly on other series, containing the powers of the sines or cosines of an arc in arithmetical progression in their numerators, which is a species whose sums are easily found. For the sake of brevity, I shall make use of the general term of any series with the characteristic S prefixed to it to denote that series. Beginning then with the series $S A x_i^i \frac{(\sin i\theta)^m}{(\cos i\theta)^n}$, we observe that when m is an even number, we have

$$\begin{aligned} S A x_i^i \frac{(\sin i\theta)^m}{(\cos i\theta)^n} &= S A x_i^i \frac{\{1 - (\cos i\theta)^2\}^{\frac{m}{2}}}{(\cos i\theta)^n} = S A x_i^i \frac{1}{(\cos i\theta)^n} - \frac{m}{2} S A x_i^i \frac{1}{(\cos i\theta)^{n-2}} \\ &+ \frac{m \cdot m-2}{2 \cdot 4} S A x_i^i \frac{1}{(\cos i\theta)^{n-4}} - \&c. \end{aligned} \quad (a)$$

this series will always terminate when m is an even number; and if m is greater than n , the last term will have no cosines in its denominator: if $m=n$, the last term will be $S A x_i^i$; and if m is less than n , the last term will be $S A x_i^i \frac{1}{(\cos i\theta)^{n-m}}$; so that in all cases when m is an even number, the series in question will depend on series of the form $S A x_i^i \frac{1}{(\cos i\theta)^n}$, or on others whose sums are known.

Let us now consider the case of m = an odd number; then we have

$$\begin{aligned} S A x_i^i \frac{(\sin i\theta)^m}{(\cos i\theta)^n} &= S A x_i^i \frac{\sin i\theta \cdot \{1 - (\cos i\theta)^2\}^{\frac{m-1}{2}}}{(\cos i\theta)^n} = \\ &= S A x_i^i \frac{(\sin i\theta)}{(\cos i\theta)^n} - \frac{m-1}{2} S A x_i^i \frac{\sin i\theta}{(\cos i\theta)^{n-2}} + \frac{m-1 \cdot m-3}{2 \cdot 4} S A x_i^i \frac{\sin i\theta}{(\cos i\theta)^{n-4}} + \&c. (b) \end{aligned}$$

This series always terminates when m is an odd number; and in a similar manner, we shall find the two following:

$$\begin{aligned} S A x_i^i \frac{(\cos i\theta)^m}{(\sin i\theta)^n} &= S A x_i^i \frac{\left\{ 1 - (\sin i\theta)^2 \right\}^{\frac{m}{2}}}{(\sin i\theta)^n} \\ &= S A x_i^i \frac{1}{(\sin i\theta)^n} - \frac{m}{2} S A x_i^i \frac{1}{(\sin i\theta)^{n-2}} + \frac{m \cdot m-2}{2 \cdot 4} S A x_i^i \frac{1}{(\sin i\theta)^{n-4}} - \&c. \quad (c) \end{aligned}$$

when m is an even number, and

$$\begin{aligned} S A x_i^i \frac{(\cos i\theta)^m}{(\sin i\theta)^n} &= S A x_i^i \frac{\cos i\theta \left\{ 1 - (\sin i\theta)^2 \right\}^{\frac{m-1}{2}}}{(\sin i\theta)^n} \\ &= S A x_i^i \frac{\cos i\theta}{(\sin i\theta)^n} - \frac{m-1}{2} S A x_i^i \frac{\cos i\theta}{(\sin i\theta)^{n-2}} + \frac{m-1 \cdot m-3}{2 \cdot 4} S A x_i^i \frac{\cos i\theta}{(\sin i\theta)^{n-4}} \&c. \quad (d) \end{aligned}$$

when m is an odd number.

Let us now propose to investigate the sum of the series

$$\frac{Ax}{(\sin \theta)^n} + \frac{Ax^2}{(\sin 2\theta)^n} + \frac{Ax^3}{(\sin 3\theta)^n} \&c.$$

$$\text{Assume } \psi x = \underset{1}{Ax} + \underset{2}{Ax^2} + \underset{3}{Ax^3} + \&c.$$

Put v^{2x} for x ; then it becomes

$$\psi v^{2x} = \underset{1}{Av^{2x}} + \underset{2}{Av^{4x}} + \underset{3}{Av^{6x}} + \&c.$$

Integrate both sides, observing that $\Sigma v^{2ix} = \frac{v^{2ix}}{v^2 - 1}$; then we have

$$\Sigma \psi v^{2x} = \underset{1}{A} \frac{v^{2x}}{v^2 - 1} + \underset{2}{A} \frac{v^{4x}}{v^4 - 1} + \underset{3}{A} \frac{v^{6x}}{v^6 - 1} + \&c.$$

Integrate again, and after the n^{th} integration we shall have

$$\Sigma^n \psi v^{2x} = \underset{1}{A} \frac{v^{2x}}{(v^2 - 1)^n} + \underset{2}{A} \frac{v^{4x}}{(v^4 - 1)^n} + \underset{3}{A} \frac{v^{6x}}{(v^6 - 1)^n} + \&c.$$

Now let $v = \cos \theta \pm \sqrt{-1} \sin \theta$; then our equation becomes

$$(2\sqrt{-1})^n \Sigma^n \psi^{2x} = \underset{1}{A} \frac{v^{2x-n}}{(\sin \theta)^n} + \underset{2}{A} \frac{v^{4x-2n}}{(\sin 2\theta)^n} + \underset{3}{A} \frac{v^{6x-3n}}{(\sin 3\theta)^n} + \&c.$$

Put $x + \frac{n}{2}$ for x , and we have

$$(2\sqrt{-1})^n \Sigma^n \psi v^{2x+n} = \underset{1}{A} \frac{v^{2x}}{(\sin \theta)^n} + \underset{2}{A} \frac{v^{4x}}{(\sin 2\theta)^n} + \underset{3}{A} \frac{v^{6x}}{(\sin 3\theta)^n} + \&c.$$

$$= A \frac{x}{(\sin \theta)^n} + A \frac{x^2}{(\sin 2\theta)^n} + A \frac{x^3}{(\sin 3\theta)^n} + \&c. \quad (1)$$

If after the integration we had put \bar{v} instead of v , and then $v = \cos \theta \pm \sqrt{-1} \sin \theta$, we should have found

$$(-2\sqrt{-1})^n \sum \bar{v}^n \psi^{\bar{v}^n - n} = A \frac{\bar{v}^{-2n}}{(\sin \theta)^n} + A \frac{\bar{v}^{-4n}}{(\sin 2\theta)^n} + A \frac{\bar{v}^{-6n}}{(\sin 3\theta)^n} + \&c. \\ A \frac{\bar{x}^{-1}}{(\sin \theta)^n} + A \frac{\bar{x}^{-2}}{(\sin 2\theta)^n} + A \frac{\bar{x}^{-3}}{(\sin 3\theta)^n} + \quad (2)$$

Neither of the integrals exhibited in (1) and (2) are integrable in the most simple cases, and it is only by their combination that I have been able to obtain the sums of any series. Let us suppose $A=1$, $A=-1$, $A=1$, $A=-1$, &c. then

$$\psi v^{2n+n} = \frac{v^{2n+n}}{1+v^{2n+n}} \text{ and } \psi v^{-2n-n} = \frac{v^{-2n-n}}{1+v^{-2n-n}}; \text{ also let } n=1;$$

then the difference of the two series is

$$(2\sqrt{-1}) \sum \left\{ \frac{v^{2n+1}}{1+v^{2n+1}} + \frac{v^{-2n-1}}{1+v^{-2n-1}} \right\} = \frac{x-x}{\sin \theta} - \frac{x^2-x^2}{\sin 2\theta} + \frac{x^3-x^3}{\sin 3\theta} \&c.$$

but the integral on the left side of this equation becomes $(2\sqrt{-1}) \sum (1)$ which is equal to $2\sqrt{-1} (z+b)$: hence, since $z = \frac{\log x}{2 \log v}$ we have

$$2\sqrt{-1} \left\{ \frac{\log x}{2 \log v} + b \right\} = \frac{x-x}{\sin \theta} - \frac{x^2-x^2}{\sin 2\theta} + \frac{x^3-x^3}{\sin 3\theta} + \&c.$$

If $x=1$, $b=0$, and since $\log v = \theta \sqrt{-1}$, our series becomes

$$\frac{\log x}{\theta} = \frac{x-x}{\sin \theta} - \frac{x^2-x^2}{\sin 2\theta} + \frac{x^3-x^3}{\sin 3\theta} - \&c. \quad (3)$$

let $x = \cos \theta' \pm \sqrt{-1} \sin \theta'$; then, since $\log x$ will become $\theta' \sqrt{-1}$, by dividing both sides by $2\sqrt{-1}$, we shall have

$$\frac{\theta'}{\theta} = \frac{\sin \theta'}{\sin \theta} - \frac{\sin 2\theta'}{\sin 2\theta} + \frac{\sin 3\theta'}{\sin 3\theta} - \&c. \quad (4)$$

The series (3) is integrable when multiplied by $\frac{dx}{x}$, and this operation may be repeated any number of times; the first

operation produces

$$\frac{(\log x)^2}{2\theta} \pm c = \frac{x+x^{-1}}{1 \sin \theta} - \frac{x^2+x^{-2}}{2 \sin 2\theta} - \frac{x^3+x^{-3}}{3 \sin 3\theta} + \&c.$$

the value of the constant c , which is equal to the series

$$c = 2 \left\{ \frac{1}{1 \sin \theta} - \frac{1}{2 \sin 2\theta} + \frac{1}{3 \sin 3\theta} - \&c. \right\}$$

cannot be determined from this equation, but by a second multiplication by $\frac{dx}{x}$ and again integrating, it may readily be found: this second operation gives

$$\frac{(\log x)^3}{2 \cdot 3 \cdot \theta} + \frac{\log x}{1} c + c = \frac{x-x^{-1}}{1^2 \sin \theta} - \frac{x^2-x^{-2}}{2^2 \sin 2\theta} + \frac{x^3-x^{-3}}{3^2 \sin 3\theta} - \&c.$$

If $x=1$, $c=0$, put $x = \cos \theta + \sqrt{-1} \sin \theta$, then we have

$$\frac{(\theta \sqrt{-1})^3}{2 \cdot 3 \cdot \theta} + \frac{\theta \sqrt{-1}}{1} c = 2\sqrt{-1} \left\{ \frac{1}{1^2} - \frac{1}{2^2} + \frac{1}{3^2} - \&c. \right\} = 2\sqrt{-1} S \frac{\pm 1}{i^2}^*$$

From this equation the value of c may be found; it is

$$c = \frac{1}{\theta} \left\{ \frac{\theta^2}{6} + 2 S \frac{\pm 1}{i^2} \right\}$$

The value of c thus found, we have the series

$$\frac{(\log x)^2}{2 \cdot \theta} + \frac{1}{\theta} \left\{ \frac{\theta^2}{6} + 2 S \frac{\pm 1}{i^2} \right\} = \frac{x+x^{-1}}{1 \sin \theta} - \frac{x^2+x^{-2}}{2 \sin 2\theta} + \frac{x^3+x^{-3}}{3 \sin 3\theta} - \&c. \quad (5)$$

and

$$\frac{(\log x)^3}{1 \cdot 2 \cdot 3 \cdot \theta} + \frac{\log x}{\theta} \left\{ \frac{\theta^2}{6} + 2 S \frac{\pm 1}{i^2} \right\} = \frac{x-x^{-1}}{1^2 \sin \theta} - \frac{x^2-x^{-2}}{2^2 \sin 2\theta} + \frac{x^3-x^{-3}}{3^2 \sin 3\theta} - \&c. \quad (6)$$

In the first of these put $x = \cos \theta + \sqrt{-1} \sin \theta$, and it be-

$$\text{comes} \quad -\frac{\theta}{6} + \frac{1}{\theta} S \frac{\pm 1}{i^2} = \frac{\cot \theta}{1} - \frac{\cot 2\theta}{2} + \frac{\cot 3\theta}{3} - \&c. \quad (7)$$

* Throughout the course of this Paper I shall have continual occasion to employ the series $\frac{1}{1^{2n}} - \frac{1}{2^{2n}} + \frac{1}{3^{2n}} - \&c.$; they can always be expressed by means of the numbers of BERNOULLI, and the powers of π , and for the sake of brevity I shall always denote them by $S \frac{\pm 1}{i^{2n}}$.

By continuing to multiply (3) by $\frac{dx}{x}$ and integrating, it is easy to perceive that we should arrive at the two following theorems—

$$\begin{aligned} & \frac{(\log x)^{2k}}{1.2...2k.0} + \frac{(\log x)^{2k-2}}{1.2...2k-2} c + \&c. + c_{2k-1} = \\ & \frac{x+x^{-1}}{1^{2k-1} \sin \theta} - \frac{x^2+x^{-2}}{2^{2k-1} \sin 2\theta} + \frac{x^3+x^{-3}}{3^{2k-1} \sin 3\theta} - \&c. \quad (8) \\ & \frac{(\log x)^{2k+1}}{1.2...2k+1.0} + \frac{(\log x)^{2k-1}}{1.2...2k-1} c + \&c. + \frac{\log x}{1} c_{2k-1} = \\ & \frac{x-x^{-1}}{1^{2k} \sin \theta} - \frac{x^2-x^{-2}}{2^{2k} \sin 2\theta} + \frac{x^3-x^{-3}}{3^{2k} \sin 3\theta} - \&c. \end{aligned}$$

the constants $c, c, c, \&c.$ may easily be determined from each other, the value of c has been already found, that of c and c are as follows:

$$\begin{aligned} c_3 &= \frac{2}{\theta} S \frac{\pm 1}{i^4} + \frac{2\theta}{6} S \frac{\mp 1}{i^2} + \frac{7\theta^3}{360} \\ c_5 &= \frac{2}{\theta} S \frac{\pm 1}{i^6} + \frac{2\theta}{6} S \frac{\pm 1}{i^4} + 2 \frac{7\theta^3}{360} S \frac{\pm 1}{i^2} + \theta^5 \frac{31}{5040} \end{aligned}$$

a variety of series are deducible from those of (8); I shall only mention two of them;

$$\begin{aligned} & \frac{\cot \theta}{1^{2k-1}} - \frac{\cot 2\theta}{2^{2k-1}} + \frac{\cot 3\theta}{3^{2k-1}} - \&c. \\ \text{and} \quad & \frac{1}{1^{2k}} \cdot \frac{\sin \theta'}{\sin \theta} - \frac{1}{2^{2k}} \cdot \frac{\sin 2\theta'}{\sin 2\theta} + \frac{1}{3^{2k}} \cdot \frac{\sin 3\theta'}{\sin 3\theta} - \&c. \end{aligned}$$

Returning to the formulæ (1) and (2) by addition and subtraction, we shall have when n is even

$$\begin{aligned} & (2\sqrt{-1})^n \sum^n \left\{ \psi v^{2z+n} + (-1)^n \psi v^{-2z-n} \right\} = \\ & A \frac{x+x^{-1}}{1 (\sin \theta)^n} + A \frac{x^2+x^{-2}}{2 (\sin 2\theta)^n} + A \frac{x^3+x^{-3}}{3 (\sin 3\theta)^n} + \&c. \end{aligned}$$

and when n is an odd number,

$$\begin{aligned} (2\sqrt{-1})^n \sum^n \{ \psi v^{2x+n} + (-1)^n \psi v^{-2x-n} \} = \\ = A \frac{x-x^{-1}}{(\sin \theta)^n} + A \frac{x^2-x^{-2}}{(\sin 2\theta)^n} + A \frac{x^3-x^{-3}}{(\sin 3\theta)^n} + \&c. \end{aligned}$$

From these expressions it appears, that the reason why we have succeeded in the integrations is, because we had so assumed ψ , that the sum, $\psi v^{2x+n} + \psi v^{-2x-n}$ is a constant quantity; the same success must follow whenever this condition is fulfilled: and hence, we have a method of discovering the sums of a great variety of series, containing the powers of the sines of arcs in arithmetical progression in their denominators, by solving the functional equation $\psi v^{2x+n} + \psi v^{-2x-n} = c$. This is fortunately one of a class whose general solution I have arrived at,* it is

$$\psi v^{2x+n} = \frac{c \phi v^{2x+n}}{\phi v^{2x+n} + \phi v^{-2x-n}} \text{ or } \psi x = \frac{c \phi x}{\phi x + \phi \frac{1}{x}}$$

In the example I have employed n was supposed equal to unity; if this is not the case, we should have found

$$(2\sqrt{-1})^n \sum^n (1) = \frac{x \pm x^{-1}}{(\sin \theta)^n} - \frac{x^2 \pm x^{-2}}{(\sin 2\theta)^n} + \frac{x^3 \pm x^{-3}}{(\sin 3\theta)^n} - \&c.$$

If in the functional equation we put $c = 1$, and $\phi x = \tan^{-1} x$, then we have $\psi x = \frac{2}{\pi} \tan^{-1} x$, and

$$(2\sqrt{-1})^n \sum^n (1) = \frac{2}{\pi} \left\{ \frac{x \pm x^{-1}}{1 (\sin \theta)^n} - \frac{x^2 \pm x^{-2}}{3 (\sin 3\theta)^n} + \frac{x^3 \pm x^{-3}}{5 (\sin 5\theta)^n} - \&c. \right\}$$

the upper or under sign being used as n , is even or odd; if $n = 1$, the constant is zero; and we have

$$\frac{\pi \log x}{2\theta} = \frac{x-x^{-1}}{1 \sin \theta} - \frac{x^3-x^{-3}}{3 \sin 3\theta} + \frac{x^5-x^{-5}}{5 \sin 5\theta} - \&c. \quad (9)$$

* See Philosophical Transactions for 1817, p. 202.

this may be multiplied by $\frac{dx}{x}$, and integrated any number of times in the same manner as (3), and the results would be

$$\begin{aligned} \frac{\pi}{2\theta} \cdot \frac{(\log x)^{2k}}{1.2...2k} + \frac{(\log x)^{2k-2}}{1.2...2k-2} c + \&c. + c_{2k-1} &= \\ &= \frac{\pi}{1^{2k} \sin \theta} - \frac{x^3 + x^{-3}}{3^{2k} \sin 3\theta} + \frac{x^5 + x^{-5}}{5^{2k} \sin 5\theta} - \&c. \quad (1,1) \\ \frac{\pi}{2\theta} \cdot \frac{(\log x)^{2k+1}}{1.2...2k+1} + \frac{(\log x)^{2k-1}}{1.2...2k-1} c + \&c. + \frac{\log x}{1} c_{2k-1} &= \\ &= \frac{x - x^{-1}}{1^{2k+1} \sin \theta} - \frac{x^3 - x^{-3}}{3^{2k+1} \sin 3\theta} + \frac{x^5 - x^{-5}}{5^{2k+1} \sin 5\theta} - \&c. \end{aligned}$$

and these constants may be determined one from the other in the same manner as the former. I shall only give the value of the first, in order to compare the value of the series to which it is equal, with the sum of the same series deduced in another manner.

$$c = \frac{\pi\theta}{12} + \frac{2}{\theta} S \frac{\pm 1}{2i+1} = 2 \left\{ \frac{1}{1^2 \sin \theta} - \frac{1}{3^2 \sin 3\theta} + \frac{1}{5^2 \sin 5\theta} - \&c. \right\} (1,2)$$

In order to ascertain the sums of series which contain cosines in their denominators, we must use an artifice which I shall now explain.

Assuming as before $\psi x = Ax + Ax^2 + Ax^3 + \&c$, and putting v^{2x} for x , we have

$$\psi v^{2x} = Av^{2x} + Av^{4x} + Av^{6x} + \&c.$$

If we were now to integrate this, we should introduce into the denominator of any term v^{2i-1} ; but we want to introduce the same expression with the signs of both terms positive. If we multiply both sides by $(-1)^x$ and then integrate, we shall have first

$$(-1)^x \psi v^{2x} = Av^{2x} (-1)^x + Av^{4x} (-1)^x + Av^{6x} (-1)^x + \&c.$$

And since $\Delta (-1)^z v^{2iz} = -(-1)^z v^{2iz+2i} - (-1)^z v^{2iz} = -(v^{2i} + 1)(-1)^z v^{2iz}$ we find

$$\Sigma (-1)^z v^{2iz} = -\frac{v^{2iz}}{v^{2i} + 1}$$

Integrating each term separately, we have

$$\Sigma (-1)^z \psi v^{2z} = -\left\{ A_1 \frac{v^{2z}(-1)^z}{v^2 + 1} + A_2 \frac{v^{4z}(-1)^z}{v^4 + 1} + A_3 \frac{v^{6z}(-1)^z}{v^6 + 1} + \&c. \right\}$$

Let this integration be repeated n times, it will give

$$(-1)^n \Sigma^n (-1)^z \psi v^{2z} = A_1 \frac{v^{2z}(-1)^z}{(v^2 + 1)^n} + A_2 \frac{v^{4z}(-1)^z}{(v^4 + 1)^n} + A_3 \frac{v^{6z}(-1)^z}{(v^6 + 1)^n} + \&c.$$

Let $v = \cos \theta \sqrt{-1} \sin \theta$, and $z + \frac{n}{2}$ for z ; this becomes

$$(-2)^n \Sigma^n (-1)^{z - \frac{n}{2}} \psi v^{2z+n} = (-1)^{z - \frac{n}{2}} \left\{ A_1 \frac{v^{2z}}{(\cos \theta)^n} + A_2 \frac{v^{4z}}{(\cos 2\theta)^n} + A_3 \frac{v^{6z}}{(\cos 3\theta)^n} + \&c. \right\}$$

And finally,

$$\begin{aligned} (-2)^n (-1)^z \Sigma^n (-1)^z \psi v^{2z+n} &= A_1 \frac{v^{2z}}{(\cos \theta)^n} + A_2 \frac{v^{4z}}{(\cos 2\theta)^n} + A_3 \frac{v^{6z}}{(\cos 3\theta)^n} + \&c. \\ &= A_1 \frac{x}{(\cos \theta)^n} + A_2 \frac{x^2}{(\cos 2\theta)^n} + A_3 \frac{x^3}{(\cos 3\theta)^n} + \&c. \quad (1,3) \end{aligned}$$

The integrations here indicated will, as in a former instance, generally surpass the powers of analysis in its present state; but a contrivance similar to that which has been already stated, will in many cases elude the difficulty: the artifice consists in investigating another similar series arranged according to the descending powers of the variable, integrating it in the same manner as we have that marked (1,3), and adding these two results, we shall in many cases have a function which is integrable, and the two series become equal in the case of $x=1$. By commencing with the descending

series $\psi v^{-2z} = A_1 v^{-2z} + A_2 v^{-4z} + A_3 v^{-6z} + \&c.$ multiplying by $(-1)^z$ and integrating, we shall get the expression

$$\begin{aligned} (-2)^n (-1)^z \sum^n (-1)^z \psi v^{-2z-n} &= A_1 \frac{v^{-2z}}{(\cos \theta)^n} + A_2 \frac{v^{-4z}}{(\cos 2\theta)^n} + A_3 \frac{v^{-6z}}{(\cos 3\theta)^n} - \&c. \\ &= A_1 \frac{x^{-1}}{(\cos \theta)^n} + A_2 \frac{x^{-2}}{(\cos 2\theta)^n} + A_3 \frac{x^{-3}}{(\cos 3\theta)^n} + \&c. \quad (1,4) \end{aligned}$$

There occur very few cases in which it is possible to execute the integrations in (1,3) and (1,4); by adding the two together, we have

$$\begin{aligned} (-2)^n (-1)^z \sum^n (-1)^z \{ \psi v^{2z+n} + \psi v^{-2z-n} \} &= A_1 \frac{x+x^{-1}}{(\cos \theta)^n} + A_2 \frac{x^2+x^{-2}}{(\cos 2\theta)^n} \\ &+ A_3 \frac{x^3+x^{-3}}{(\cos 3\theta)^n} + \&c. \quad (1,5) \end{aligned}$$

Here we may observe that the new series is exactly double either of the others (1,3) or (1,4) when $x=1$; also, that the integration on the left side can be executed any number of times, whenever $\psi v^{2z+n} \psi v^{-2z-n}$ is a constant quantity; the forms of the function ψ , which fulfil this condition, have already been given. Let $\psi x = \frac{x}{1+x}$, then $A_1 = 1$, $A_2 = -1$, $A_3 = 1$, &c.

and since $\psi v^{2z+n} + \psi v^{-2z-n} = 1$, we have

$$(-2)^n (-1)^z \sum^n (-1)^z = \frac{x+x^{-1}}{(\cos \theta)^n} - \frac{x^2+x^{-2}}{(\cos 2\theta)^n} + \frac{x^3+x^{-3}}{(\cos 3\theta)^n} + \&c.$$

These integrations are easily executed; and commencing with $n=1$, we have

$$-2 \cdot (-1)^z \frac{(-1)^z}{1-1} + 2b(-1)^z = 1 + 2b(-1)^z = \frac{x+x^{-1}}{\cos \theta} - \frac{x^2+x^{-2}}{\cos 2\theta} + \&c$$

In order to determine the constant b , put $x = \cos \theta + \sqrt{-1} \sin \theta$; then, since z in that case becomes $\frac{1}{2}$, we have

$$1 + 2b\sqrt{-1} = 2 - 2 + 2 - 2 + \&c. = 1$$

hence $b=0$, and we have

$$1 = \frac{x+\bar{x}}{\cos \theta} - \frac{x^2+\bar{x}^2}{\cos 2\theta} + \frac{x^3+\bar{x}^3}{\cos 3\theta} - \&c. \quad (1,6)$$

Continuing to integrate, it will be found that all the constants are zero, and we shall arrive at the following theorem;

$$1 = \frac{x+\bar{x}}{(\cos \theta)^n} - \frac{x^2+\bar{x}^2}{(\cos 2\theta)^n} + \frac{x^3+\bar{x}^3}{(\cos 3\theta)^n} - \&c. \quad (1,7)$$

Let $x = -x$, then it becomes

$$-1 = \frac{x+\bar{x}}{(\cos \theta)^n} + \frac{x^2+\bar{x}^2}{(\cos 2\theta)^n} + \frac{x^3+\bar{x}^3}{(\cos 3\theta)^n} + \&c. \quad (1,8)$$

Putting $x=1$ in both these, we have

$$\frac{1}{2} = \frac{1}{(\cos \theta)^n} - \frac{1}{(\cos 2\theta)^n} + \frac{1}{(\cos 3\theta)^n} - \&c. \quad (1,9)$$

$$-\frac{1}{2} = \frac{1}{(\cos \theta)^n} + \frac{1}{(\cos 2\theta)^n} + \frac{1}{(\cos 3\theta)^n} + \&c. \quad (2,1)$$

I propose in the next place to determine the value of the series

$$1^{2k} \frac{1}{(\cos \theta)^n} - 2^{2k} \frac{1}{(\cos 2\theta)^n} + 3^{2k} \frac{1}{(\cos 3\theta)^n} - \&c.$$

This may be accomplished by multiplying (1,7) by $\frac{dx}{x}$, and integrating; this operation, being performed on it 2^k times, will produce the series whose sum is required; the first integration gives

$$\frac{\log x}{1} + c = \frac{x-\bar{x}}{1(\cos \theta)^n} - \frac{x^2-\bar{x}^2}{2(\cos 2\theta)^n} + \frac{x^3-\bar{x}^3}{3(\cos 3\theta)^n} - \&c.$$

If $x=1$, $c=0$, the second operation gives

$$\frac{(\log x)^2}{1 \cdot 2} + c = \frac{x+\bar{x}}{1^2(\cos \theta)^n} - \frac{x^2+\bar{x}^2}{2^2(\cos 2\theta)^n} - \&c.$$

$$\text{If } x=1 \quad c_{2,n} = \frac{2}{1^2(\cos \theta)^n} - \frac{2}{2^2(\cos 2\theta)^n} + \&c.$$

In order to determine $c_{2,n}$ put $x = \cos \theta + \sqrt{-1} \sin \theta$, then we have

$$-\frac{\theta^2}{2} + c_{2,n} = \frac{2}{1^2(\cos \theta)^{n-1}} - \frac{2}{2^2(\cos \theta)^{n-1}} + \&c. = c_{2,n-1}$$

The equation $c_{2,n} - c_{2,n-1} = \frac{\theta^2}{2}$ being integrated, gives

$$c_{2,n} = \frac{n}{2} \frac{\theta^2}{2} + b$$

If $n=0$ $c_{2,0} = b = \frac{2}{1^2} - \frac{2}{2^2} + \frac{2}{3^2} - \&c. = 2S \frac{\pm 1}{i^2}$

Hence $c_{2,n} = \frac{n}{1} \cdot \frac{\theta^2}{2} + 2S \frac{\pm 1}{i^2}$ and

$$\frac{(\log x)^2}{1.2} + \frac{n}{1} \cdot \frac{\theta^2}{2} + 2S \frac{\pm 1}{i^2} = \frac{x+x^{-1}}{1^2(\cos \theta)^n} - \frac{x^2+x^{-2}}{2^2(\cos 2\theta)^n} + \frac{x^3+x^{-3}}{3^2(\cos 3\theta)^n} \&c. \quad (2,2)$$

These integrations being repeated, we shall arrive at the two following expressions:

$$\begin{aligned} & \frac{(\log x)^{2k}}{1.2 \dots 2k} + \frac{(\log x)^{2k-2}}{1.2 \dots 2k-2} c_{2,n} + \&c. + \frac{(\log x)^2}{1.2} c_{2k-2,n} + c_{2k,n} = \\ & = \frac{x+x^{-1}}{1^{2k}(\cos \theta)^n} - \frac{x^2+x^{-2}}{2^{2k}(\cos 2\theta)^n} + \frac{x^3+x^{-3}}{3^{2k}(\cos 3\theta)^n} + \&c. \quad (2,3) \\ & \frac{(\log x)^{2k+1}}{1.2 \dots 2k+1} + \frac{(\log x)^{2k-1}}{1.2 \dots 2k-1} c_{2,n} + \&c. c_{2k,n} = \\ & \frac{x-x^{-1}}{1^{2k+1}(\cos \theta)^n} - \frac{x^2-x^{-2}}{2^{2k+1}(\cos 2\theta)^n} + \frac{x^3-x^{-3}}{3^{2k+1}(\cos 3\theta)^n} - \&c. \end{aligned}$$

It now becomes necessary to determine the value of $c_{2k,n}$, which is equal to twice the sum of the series we are investigating; for if $x=1$

$$c_{2k,n} = \frac{2}{1^{2k}(\cos \theta)^n} - \frac{2}{2^{2k}(\cos 2\theta)^n} + \frac{2}{3^{2k}(\cos 3\theta)^n} - \&c.$$

For this purpose put in the first of the equations (2,3) $x = \cos \theta + \sqrt{-1} \sin \theta$, then the series on the right hand is equal to $c_{2k,n-1}$, and we have for determining $c_{2k,n}$ the equation of finite differences.

$$\frac{(\theta\sqrt{-1})^{2k}}{1.2 \dots 2k} + \frac{(\theta\sqrt{-1})^{2k-2}}{1.2 \dots 2k-2} c_{2,n} + \frac{(\theta\sqrt{-1})^{2k-4}}{1.2 \dots 2k-4} c_{4,n} + \&c. + c_{2k,n} = c_{2k,n-1}$$

In order to integrate this equation, let us suppose $c_{2k,n}$ to represent the co-efficient of r^{2k} in the developement of $\frac{f(r)}{(ar)^n}$

where $\alpha(r) = 1 + Ar^2 + Br^4 + Cr^6 + \&c.$

then $\frac{f(r)}{(ar)^n} = 1 + r^2 c_{2,n} + r^4 c_{4,n} + r^6 c_{6,n} + \&c.$

If this be multiplied by ar , it becomes

$$\begin{aligned} \frac{f(r)}{(ar)^{n-1}} &= 1 + r^2 c_{2,n} + r^4 c_{4,n} + r^6 c_{6,n} + \\ &\quad r^2 A + r^4 A c_{2,n} + r^6 A c_{4,n} \\ &\quad r^4 B + r^6 B c_{2,n} \\ &\quad + r^6 C \end{aligned}$$

But the co-efficient of r^{2k} in this series is equal to $c_{2k,n-1}$ hence

$$c_{2k,n} + A c_{2k-2,n} + B c_{2k-4,n} + \&c. = c_{2k,n-1}$$

This equation will become the one in question, if we make

$$A = \frac{(\theta\sqrt{-1})^2}{1.2} B = \frac{(\theta\sqrt{-1})^4}{1.2.3.4} C = \frac{(\theta\sqrt{-1})^6}{1.2..6} \&c.$$

This produces

$$c_{2k,n} + \frac{(\theta\sqrt{-1})^2}{1.2} c_{2k-2,n} + \frac{(\theta\sqrt{-1})^4}{1.2.3.4} c_{2k-4,n} + \&c. = c_{2k,n-1}$$

We have now only to determine the form of $f(r)$, and this may be easily accomplished since the values of $c_{2k,0}$ are known; for if we put $n = 0$

$$f(r) = 1 + r^2 c_{2,0} + r^4 c_{4,0} + r^6 c_{6,0} + \&c.$$

$$= 1 + 2r^2 S \frac{\pm 1}{i^2} + 2r^4 S \frac{\pm 1}{i^4} + 2r^6 S \frac{\pm 1}{i^6} + \&c.$$

Therefore $c_{2k,n}$ is equal to the co-efficient of r^{2k} in the developement of

$$\frac{1 + 2r^2 S \frac{\pm 1}{i^2} + 2r^4 S \frac{\pm 1}{i^4} + \&c.}{(\cos r\theta)^n}$$

Or if $(\cos r\theta)^{-n} = 1 + A' \theta^2 r^2 + B' \theta^4 r^4 + C' \theta^6 r^6 + \&c.$ then

$$c_{2k,n} = 2S \frac{\pm 1}{i^{2k}} + 2A'_n \theta^2 S \frac{\pm 1}{i^{2k-2}} + 2B'_n \theta^4 S \frac{\pm 1}{i^{2k-4}} + 2C'_n \theta^6 S \frac{\pm 1}{i^{2k-6}} + \&c.$$

The quantity $c_{2k,n}$ may now be considered as completely determined, since it only depends on the co-efficients of $(\cos \theta)^{-n}$, and the series marked by $S \frac{\pm 1}{i^{2k}}$, both which quantities are known; the latter being given by the powers of π and numbers of BERNOUILLI, whilst the values of the former in functions of n are given by LEGENDRE, in his *Exercice de Calcul Integral*, vol. iii. art. 149, 155.

In (2,3) let $x = 1$, and we have

$$\frac{1}{2} c_{2k,n} = \frac{1}{1^{2k}(\cos \theta)^n} - \frac{1}{2^{2k}(\cos 2\theta)^n} + \frac{1}{3^{2k}(\cos 3\theta)^n} - \&c. \quad (2,4)$$

And if we put $x = v$ in the other series, it becomes

$$\begin{aligned} \frac{\theta}{2} \left\{ \frac{(-\theta^2)^k}{1.2 \dots 2k+1} + \frac{(-\theta^2)^{k-1}}{1.2 \dots 2k-1} c_{2,n} + \&c. + \frac{(-\theta^2)}{1.2.3} c_{2k-2,n} + \frac{1}{2} c_{2k,n} \right\} = \\ = \frac{\sin \theta}{1^{2k+1}(\cos \theta)^n} - \frac{\sin 2\theta}{2^{2k+1}(\cos 2\theta)^n} + \frac{\sin 3\theta}{3^{2k+1}(\cos 3\theta)^n} - \&c. \quad (2,5) \end{aligned}$$

If $n = 1$ this series becomes

$$\frac{\tan \theta}{1^{2k+1}} - \frac{\tan 2\theta}{2^{2k+1}} + \frac{\tan 3\theta}{3^{2k+1}} - \&c. \quad (2,6)$$

The series (2,4) may be changed into another, which contains sines both in the numerator and the denominator, for it is equal to

$$\frac{1}{2} c_{2k,n} = \frac{1}{1^{2k}} \left(\frac{\sin \theta}{\sin \theta \cos \theta} \right)^n - \frac{1}{2^{2k}} \left(\frac{\sin 2\theta}{\sin 2\theta \cos 2\theta} \right)^n + \frac{1}{3^{2k}} \left(\frac{\sin 3\theta}{\sin 3\theta \cos 3\theta} \right)^n - \&c.$$

But this becomes, since $\sin \theta \cos \theta = \frac{1}{2} \sin 2\theta$

$$\frac{1}{2^{n+1}} c_{2k,n} = \frac{1}{1^{2k}} \left(\frac{\sin \theta}{\sin 2\theta} \right)^n - \frac{1}{2^{2k}} \left(\frac{\sin 2\theta}{\sin 4\theta} \right)^n + \frac{1}{3^{2k}} \left(\frac{\sin 3\theta}{\sin 6\theta} \right)^n - \&c. \quad (2,7)$$

By applying the theorems (a), (b), (c), and (d) to the series whose sums we have now investigated, we shall arrive at the value of many others which contain the powers of tangents

and co-tangents in arithmetical progression, thus (1,9) combined with (a) will produce

$$\frac{1}{2} \left\{ 1 - \frac{k}{1} + \frac{k.k-1}{1.2} - \&c. \right\} = \frac{(\sin \theta)^{2k}}{(\cos \theta)^n} - \frac{(\sin 2\theta)^{2k}}{(\cos 2\theta)^n} + \frac{(\sin 3\theta)^{2k}}{(\cos 3\theta)^n} - \&c.$$

But the left side of this equation is equal to $\frac{1}{2} (1-1)^k = 0$.

Hence

$$0 = \frac{(\sin \theta)^{2k}}{(\cos \theta)^n} - \frac{(\sin 2\theta)^{2k}}{(\cos 2\theta)^n} + \frac{(\sin 3\theta)^{2k}}{(\cos 3\theta)^n} - \&c. \quad (2,8)$$

And if $n = 2k$, this produces a series of tangents

$$0 = (\tan \theta)^{2k} - (\tan 2\theta)^{2k} + (\tan 3\theta)^{2k} - \&c. \quad (2,9)$$

By means of the theorems already referred to, we may introduce into the numerators of each term of the series (2,4) the even powers of the sines of the same arcs whose co-sines occur in the denominator: putting $l = \frac{m}{2}$, we shall have

$$\begin{aligned} \frac{1}{2} \left\{ c_{2k,n} - \frac{l}{1} c_{2k,n-2} + \frac{l.l-1}{1.2} c_{2k,n-4} - \&c. \right\} = \\ = \frac{(\sin \theta)^{2l}}{1^{2k}(\cos \theta)^n} - \frac{(\sin 2\theta)^{2l}}{2^{2k}(\cos 2\theta)^n} + \frac{(\sin 3\theta)^{2l}}{3^{2k}(\cos 3\theta)^n} - \&c. \end{aligned} \quad (3,1)$$

And if $n = 2l$, this becomes

$$\begin{aligned} \frac{1}{2} \left\{ c_{2k,2l} - \frac{l}{1} c_{2k,2l-2} + \frac{l.l-1}{1.2} c_{2k,2l-4} - \&c. \right\} = \\ = \frac{(\tan \theta)^{2l}}{1^{2k}} - \frac{(\tan 2\theta)^{2l}}{2^{2k}} + \frac{(\tan 3\theta)^{2l}}{3^{2k}} - \&c. \end{aligned} \quad (3,2)$$

If we call the sum of the series (2,5) $A_{k,n}$, and if we apply to it the formula (b), we shall have

$$\begin{aligned} A_{k,n} - \frac{l}{1} A_{k,n-2} + \frac{l.l-1}{1.2} A_{k,n-4} - \&c. = \\ = \frac{(\sin \theta)^{2l+1}}{1^{2k+1}(\cos \theta)^n} - \frac{(\sin 2\theta)^{2l+1}}{2^{2k+1}(\cos 2\theta)^n} + \frac{(\sin 3\theta)^{2l+1}}{3^{2k+1}(\cos 3\theta)^n} - \&c. \end{aligned} \quad (3,3)$$

And if $n = 2l + 1$, this becomes

$$\begin{aligned} A_{k,2l+1} - \frac{l}{1} A_{k,2l-1} + \&c. = \\ = \frac{(\tan \theta)^{2l+1}}{1^{2k+1}} - \frac{(\tan 2\theta)^{2l+1}}{2^{2k+1}} + \frac{(\tan 3\theta)^{2l+1}}{3^{2k+1}} - \&c. \end{aligned} \quad (3,4)$$

In the equation (1.4) if we make $\psi x = \tan^{-1} x$, we shall find

$$\begin{aligned} (-2)^n (-1)^z \sum^n (-1)^{-z} \left\{ \tan^{-1} v^{2z+n} + \tan^{-1} v^{-2z-n} \right\} = \\ = (-2)^n (-1)^z \sum^n (-1)^{-z} \cdot \frac{\pi}{4} = \frac{x+x^{-1}}{1(\cos \theta)^n} - \frac{x^3+x^{-3}}{3(\cos 3\theta)^n} + \frac{x^5+x^{-5}}{5(\cos 5\theta)^n} - \&c. \end{aligned}$$

If the integrations here indicated are performed, it will be found that all the constants vanish, and ultimately that

$$\frac{\pi}{2} = \frac{x+x^{-1}}{1(\cos \theta)^n} - \frac{x^3+x^{-3}}{3(\cos 3\theta)^n} + \frac{x^5+x^{-5}}{5(\cos 5\theta)^n} - \&c. \quad (3.5)$$

If $x=1$

$$\frac{\pi}{4} = \frac{1}{1(\cos \theta)^n} - \frac{1}{3(\cos 3\theta)^n} + \frac{1}{5(\cos 5\theta)^n} - \&c. \quad (3.6)$$

If we multiply (3.5) by $\frac{dx}{x}$ and integrate, we have

$$\frac{\pi}{2} \log x + C = \frac{x-x^{-1}}{1^2(\cos \theta)^n} - \frac{x^3-x^{-3}}{3^2(\cos 3\theta)^n} + \frac{x^5-x^{-5}}{5^2(\cos 5\theta)^n} - \&c. \quad (3.7)$$

If $x=1$ $C=0$, let $x = \cos \theta' + \sqrt{-1} \sin \theta'$, then we have

$$\frac{\pi \theta'}{2} = \frac{\sin \theta'}{1^2(\cos \theta)^n} - \frac{\sin 3\theta'}{3(\cos 3\theta)^n} + \frac{\sin 5\theta'}{5(\cos 5\theta)^n} - \&c. \quad (3.8)$$

The equation (3.7) may be multiplied any number of times by $\frac{dx}{x}$, and integrated; and the constants thus introduced may be determined in the same manner as those of the equations (2.3); these operations will give the values of series of the following form:

$$\begin{aligned} \frac{x+x^{-1}}{1^{2k+1}(\cos \theta)^n} - \frac{x^3+x^{-3}}{3^{2k+1}(\cos 3\theta)^n} + \frac{x^5+x^{-5}}{5^{2k+1}(\cos 5\theta)^n} - \&c, \\ \frac{x-x^{-1}}{1^{2k}(\cos \theta)^n} - \frac{x^3-x^{-3}}{3^{2k}(\cos 3\theta)^n} + \frac{x^5-x^{-5}}{5^{2k}(\cos 5\theta)^n} - \&c. \end{aligned}$$

Numerous other series might be found by satisfying the equation $\psi x + \psi \frac{1}{x} = 1$, whose sums would be given by this process; and if instead of putting $\frac{1}{x}$ for x , we had substituted αx for x , where the function α is determined by the equation $\alpha^2 x = x$, many others would be discovered. This artifice is

only a particular case of a much more general principle, which is of use in discovering certain values of the variable, in which a series admits of summation, but which generally is not expressible in finite terms. The principle is as follows: let K denote any operation, such as integration, either with respect to differential or finite differences, or any other operation, provided only $K(X + Y) = KX + KY$. Now let

$$K\psi x = A_1x + A_2x^2 + A_3x^3 +$$

Put $\alpha x, \alpha^2x, \dots \alpha^{n-1}x$ for x , and the results will be

$$K\psi \alpha x = A_1\alpha x + A_2\alpha x^2 + A_3\alpha x^3 +$$

$$K\psi \alpha^2x = A_1\alpha^2x + A_2\alpha^2x^2 + A_3\alpha^3x^3 +$$

&c.

&c.

$$K\psi \alpha^{n-1}x = A_1\alpha^{n-1}x + A_2\alpha^{n-1}x^2 + A_3\alpha^{n-1}x^3 +$$

By adding all these together, we shall have $K\{\psi x + \psi \alpha x + \psi \alpha^2x + \dots \psi \alpha^{n-1}x\}$ equal to a series whose general term is $A_n\{x + \alpha x + \alpha^2x + \dots + \alpha^{n-1}x\}$. Now supposing we cannot perform the operation denoted by K on the function ψx , yet if ψ is of such a form that $\psi x + \psi \alpha x + \dots + \psi \alpha^{n-1}x$ is equal to a function on which the operation K can be executed, then calling this new function ψ_1 , we shall have

$$K\psi_1x = SA_i\{x^i + \alpha x^i + \dots \alpha^{n-1}x^i\}$$

And if α is such a function that $\alpha^n x = x$, a great variety of forms for ψ may be found, which will satisfy that condition. Now let x be determined by the equation $x = \alpha x$, and r being any root of this, we have $r = \alpha r = \alpha^2 r = \dots = \alpha^{n-1} r$.

Consequently our equation becomes

$$K\psi_1x = nSA_i r^i = n\{A_1r + A_2r^2 + A_3r^3 + A_4r^4 + \dots\}$$

provided we put r for x after the operation K is executed; that is, we have found the values of the series

$$A_1x + A_2x^2 + A_3x^3 + \&c.$$

in the particular cases of x which satisfy the equation $ax = x$.

PART II.

I shall now explain another method of deducing the sums of a variety of series, which comprehend amongst them all those which are contained in the former part of this paper; it rests fundamentally on the following formulæ, which have long been known:

$$0 = 1^{2n} - 2^{2n} + 3^{2n} - 4^{2n} + \&c.$$

$$\frac{1}{2} = \cos \theta - \cos 2\theta + \cos 3\theta - \&c.$$

$$\frac{1}{2} = \cos \theta + \cos 2\theta + \cos 3\theta + \&c.$$

$$0 = 1^{2n+1} - 3^{2n+1} + 5^{2n+1} - \&c.$$

It is unnecessary to give proofs of these and other similar ones which have been frequently noticed, as they may be very easily demonstrated.

Let fx be any function of x developeable in even powers of x , then $f(x) = A + Bx^2 + Cx^4 + Dx^6 + \&c.$

Divide both sides by x^{2k} , then it becomes

$$\frac{f(x)}{x^{2k}} = \frac{A}{x^{2k}} + \frac{B}{x^{2k-2}} + \&c. + K + Lx^2 + Mx^4 + \&c.$$

For x , put successively $1x$, $2x$, $3x$, $4x$, $\&c.$ and let the alternate series be taken negatively; these being arranged under each other, we have

$$\begin{aligned} + \frac{f(x)}{1^{2k} x^{2k}} &= + \frac{A}{1^{2k} x^{2k}} + \frac{B}{1^{2k-2} x^{2k-2}} + \&c. + K + Lx^2 + Mx^4 + \\ - \frac{f(2x)}{2^{2k} x^{2k}} &= - \frac{A}{2^{2k} x^{2k}} - \frac{B}{2^{2k-2} x^{2k-2}} - \&c. - K - Lx^2 - Mx^4 - \end{aligned}$$

$$\begin{aligned}
 + \frac{f(3x)}{3^{2k} x^{2k}} &= + \frac{A}{3^{2k} x^{2k}} + \frac{B}{3^{2k-2} x^{2k-2}} + \&c. + K + Lx^2 3^2 + Mx^4 3^4 + \\
 - \frac{f(4x)}{4^{2k} x^{2k}} &= - \frac{A}{4^{2k} x^{2k}} - \frac{B}{4^{2k-2} x^{2k-2}} - \&c. - K - Lx^2 4^2 - Mx^4 4^4 - \\
 &\&c. \qquad \qquad \qquad \&c. \qquad \qquad \qquad \&c.
 \end{aligned}$$

If we add the vertical columns, we shall have on the left side of the equation the series

$$\frac{1}{x^{2k}} \left\{ \frac{f(x)}{1^{2k}} - \frac{f(2x)}{2^{2k}} + \frac{f(3x)}{3^{2k}} - \frac{f(4x)}{4^{2k}} + \&c. \right\}$$

and the right side of the equation consists of three kinds of terms, those which contain negative powers of x , one which does not contain x , and the remaining ones which contain positive powers of x . With respect to these last, they are all of the form $Qx^{2i} \{ 1^{2i} - 2^{2i} + 3^{2i} - 4^{2i} + \&c. \}$; and as the series which multiplies Qx^{2i} is equal to zero, all those vertical columns which contain even powers of x will vanish: the term which is independent on x is

$$K - K + K - K + \&c. = \frac{1}{2}K$$

and those terms which contain negative powers of x , may be represented by the expression $S \frac{\pm 1}{i^{2k}}$. All the vertical columns being summed, we shall have the equation

$$\begin{aligned}
 \frac{f(x)}{1^{2k}} - \frac{f(2x)}{2^{2k}} + \frac{f(3x)}{3^{2k}} - \&c. &= AS \frac{\pm 1}{i^{2k}} + BS \frac{\pm 1}{i^{2k-2}} + \\
 &+ CS \frac{\pm 1}{i^{2k-4}} + \&c. + \frac{1}{2}K. \qquad (A)
 \end{aligned}$$

As the operations by which we have arrived at this expression have been given at length, it will be unnecessary to repeat them with the slight modifications which would be required for cases nearly similar. Thus, if we suppose the function fx developable according to the odd powers of x , and if we divide both sides of the equation by x^{2k+1} and

repeat the same process we have already explained, we shall arrive at the following theorem :

$$\begin{aligned} \frac{f(x)}{1^{2k+1}} - \frac{f(2x)}{2^{2k+1}} + \frac{f(3x)}{3^{2k+1}} - \frac{f(4x)}{4^{2k+1}} + \&c. = \\ = AxS \frac{\pm 1}{i^{2k}} + Bx^3S \frac{\pm 1}{i^{2k-2}} + \&c. + \frac{1}{2} K \end{aligned} \quad (B)$$

Let $f(\theta)$ be any function of θ developable in the form

$$f(\theta) = A + B \cos \theta + C \cos 2\theta + D \cos 3\theta \&c.$$

a very similar process to that which has been already explained will give the

$$f(\theta) - f(2\theta) + f(3\theta) - \&c. = \frac{f(0)}{2} \quad (C)$$

and if $f(\theta) = A \cos \theta + B \cos 2\theta + C \cos 3\theta + \&c.$ a similar course will produce the equation

$$f(\theta) + f(2\theta) + f(3\theta) + \&c. = -\frac{1}{2} f(0) \quad (D)$$

If a function is developable in even powers of x , then its second function is developable in the same manner, and so are all its higher functions; therefore if f_i and f are two functions developable in even powers of x , such that

$$fx = A' + B'x^2 + C'x^4 + \&c.$$

$$f_i f^n x = A + Bx^2 + Cx^4 + \&c.$$

Then (A) will become

$$\begin{aligned} \frac{f_i f^n(x)}{1^{2k}} - \frac{f_i f^n(2x)}{2^{2k}} + \frac{f_i f^n(3x)}{3^{2k}} - \&c. = \\ = AS \frac{\pm 1}{i^{2k}} + BS \frac{\pm 1}{i^{2k-2}} + \&c. + \frac{1}{2} K \end{aligned} \quad (E)$$

These theorems marked (A), (B), (C), and (D), although they possess a very great degree of generality, are not entirely without restriction; it appears at first sight that they are applicable to *all* functions which have the prescribed condition of being expansible in even powers of the variable; such was my opinion of them when I first discovered them; but

several results which were evidently incorrect, soon convinced me that some limitation existed, of whose nature I was not aware: it was not until some years after, that I found out the cause of the fallacies which had perplexed me; and still more recently, I discovered that the series on whose sum their truth or falsehood depended, might be expressed by a definite integral. By applying the criterion, which I shall presently explain, we cut off a great variety of series whose sums are erroneously given by the method in question; whether this criterion does not exclude some series whose sums are correctly given, is a point which I do not consider yet completely decided; the difficulties to which the application of acknowledged principles have in this instance conducted us, appear worthy of the attention of mathematicians. A more strict method might have been pursued in determining the sum of that part of the series which is neglected; but this in general leads to such differential equations, as cannot afford us much assistance. I have, however, given one example of this method, and I have shown that when the part which had been neglected, as being apparently equal to zero (but which is in fact a finite quantity,) is added to the sum furnished by the *method of expanding horizontally and summing vertically*, the true value of the series results. This confirms the explanation I have given of the reason of the apparent failure of that method.

It will be sufficient to point out the cause which leads to error, and to determine the conditions on which its existence depends for one only of the series; suitable modifications of the reasoning will readily suggest themselves for the others. I shall therefore, at present, consider the theorem (A). If

we turn to the process employed in its investigation, we may remark, that the vertical column $Lx^3 (1^3 - 2^3 + 3^3 - 4^3 + \&c.)$ has been neglected, because the series which enters into it as a factor is equal to zero; so also the vertical column $Mx^4 (1^4 - 2^4 + 3^4 - \&c.)$ is neglected for the same reason, and similarly for all the remaining vertical columns. Now, although it would be perfectly correct to omit any one, or even any finite number of these vertical columns, as being multiplied by a factor equal to zero, yet it is not legitimate to neglect an infinite number of terms, each multiplied by zero, unless it can be proved that the sum of all the terms so multiplied is not an infinite quantity: this, then, is the latent cause of the false results at which I arrived at the commencement of these enquiries. I shall now explain how they may be obviated, or rather how to assign the condition on which the truth of the theorems just deduced depend. We have considered the series of terms

$$Lx^3 (1^3 - 2^3 + 3^3 - \&c.) + Mx^4 (1^4 - 2^4 + 3^4 - \&c.) + \\ + Nx^5 (1^5 - 2^5 + 3^5 - \&c.) + \&c.$$

as equal to zero. Any one of the series which here multiply the powers of x , may be considered as arising from the series

$$1^{2n}y - 2^{2n}y^2 + 3^{2n}y^3 + \&c.$$

When $y = 1$, call this series $K_n(y)$, and instead of making $y = 1$, let $y = 1 + o$, which differs from unity by the infinitely small quantity o ; then we shall have

$$K_n(1 + o) = c_{1,n} + c_{2,n}o + c_{3,n}o^2 + \&c.$$

Where

$$c_{1,n} = 0 = 1^{2n} - 2^{2n} + 3^{2n} - \&c. \quad c_{2,n} = 1^{2n+1} - 2^{2n+1} + 3^{2n+1} - \&c.$$

by substituting this value of $K_n(1 + o)$ in the series we had neglected, we shall find

$$\begin{aligned} & \{Lx^2c_{1,1} + Mx^4c_{1,2} + Nx^6c_{1,3} + \&c.\} \\ & + o \{Lx^2c_{2,1} + Mx^4c_{2,2} + Nx^6c_{2,3} + \&c.\} \\ & + o^2 \{Lx^2c_{3,1} + Mx^4c_{3,2} + Nx^6c_{3,3} + \&c.\} \\ & + o^3 \{Lx^2c_{4,1} + Mx^4c_{4,2} + Nx^6c_{4,3} + \&c.\} \\ & + \&c. \qquad \qquad \qquad \&c. \end{aligned}$$

The first line vanishes on account of the value of $c_{1,n}$, and since o is an infinitesimal, the second line will be larger than the sum of all the rest, provided the multipliers of the powers of o are finite; if therefore the series $Lx^2c_{2,1} + Mx^4c_{2,2} + Nx^6c_{2,3} + \&c.$ is finite since it is multiplied by o , we may neglect the whole of the above expression: our next step must be to determine whether the series

$$\begin{aligned} & Lx^2 \{1^3 - 2^3 + 3^3 - \&c.\} + Mx^4 \{1^5 - 2^5 + 3^5 - \&c.\} + \\ & + Nx^6 \{1^7 - 2^7 + 3^7 - \&c.\} + \&c. \end{aligned}$$

is finite or infinite. It has been observed by EULER, that the following relations exist between the direct and reciprocal powers of the natural numbers.

$$\begin{aligned} 1 - 2 + 3 - 4 + \&c. &= + 2 \frac{1}{\pi^2} \left\{ 1 + \frac{1}{3^2} + \frac{1}{5^2} + \&c. \right\} \\ 1^3 - 2^3 + 3^3 - 4^3 + \&c. &= - 2 \frac{1 \cdot 2 \cdot 3}{\pi^4} \left\{ 1 + \frac{1}{3^4} + \frac{1}{5^4} + \&c. \right\} \\ 1^5 - 2^5 + 3^5 - 4^5 + \&c. &= + 2 \frac{1 \cdot 3 \cdot 5}{\pi^6} \left\{ 1 + \frac{1}{3^6} + \frac{1}{5^6} + \&c. \right\} \\ 1^7 - 2^7 + 3^7 - 4^7 + \&c. &= - 2 \frac{1 \cdot 3 \cdot 5 \cdot 7}{\pi^8} \left\{ 1 + \frac{1}{3^8} + \frac{1}{5^8} + \&c. \right\} \\ \&c. & \qquad \qquad \qquad \&c. \end{aligned}$$

These latter being substituted for their equals, we have

$$\begin{aligned} & - 2Lx^2 \frac{1 \cdot 2 \cdot 3}{\pi^4} \left(\frac{1}{1^4} + \frac{1}{3^4} + \frac{1}{5^4} + \&c. \right) + 2Mx^4 \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5}{\pi^6} \left(\frac{1}{1^6} + \frac{1}{3^6} + \right. \\ & \left. + \frac{1}{5^6} \&c. \right) - 2Nx^6 \frac{1 \cdot 3 \cdot 5 \cdot 7}{\pi^8} \left(\frac{1}{1^8} + \frac{1}{3^8} + \frac{1}{5^8} + \&c. \right) + \&c. \end{aligned}$$

The series which now multiplies each term is in all cases

less than 2, consequently this expression will be finite or infinite, according as the series.

$$-Lx^{\frac{1.2.3}{\pi^4}} + Mx^{\frac{1..5}{\pi^6}} - Nx^{\frac{61...7}{\pi^8}} + \&c.$$

is so or the contrary: the product $1.2...n$ may be expressed by means of a definite integral, thus:

$$1.2...n = \int dv \left(\log \frac{1}{v} \right)^n \quad \left[\begin{matrix} v=0 \\ v=1 \end{matrix} \right]$$

These products being replaced by the integral, we have

$$\frac{1}{\pi^2} \int dv \left\{ -L \left(\frac{x}{\pi} \right)^2 \left(\log \frac{1}{v} \right)^3 + M \left(\frac{x}{\pi} \right)^4 \left(\log \frac{1}{v} \right)^5 - N \left(\frac{x}{\pi} \right)^6 \left(\log \frac{1}{v} \right)^7 + \&c. \right\} \quad \left[\begin{matrix} v=0 \\ v=1 \end{matrix} \right]$$

$$\frac{1}{\pi^2} \int dv \log \frac{1}{v} \cdot \left\{ -L \left(\frac{x}{\pi} \log \frac{1}{v} \right)^2 + M \left(\frac{x}{\pi} \log \frac{1}{v} \right)^4 - N \left(\frac{x}{\pi} \log \frac{1}{v} \right)^6 + \&c. \right\}$$

Now in order to determine the sum of this series, which evidently depends on the function $f(x)$, let us assume

$$\frac{f(x)}{x^{2k}} = \frac{A}{x^{2k}} - \frac{B}{x^{2k-2}} - \&c. - K = \chi(x)$$

then we have

$$\chi(x) = Lx^2 + Mx^4 + Nx^6 + \&c.$$

And if we put $\frac{x\sqrt{-1}}{\pi} \log \frac{1}{v}$ instead of x , it becomes

$$\chi \left(\frac{x\sqrt{-1}}{\pi} \log \frac{1}{v} \right) = -L \left(\frac{x}{\pi} \log \frac{1}{v} \right)^2 + M \left(\frac{x}{\pi} \log \frac{1}{v} \right)^4 - N \left(\frac{x}{\pi} \log \frac{1}{v} \right)^6 + \&c.$$

And the sum of the series in question is

$$\frac{1}{\pi^2} \int dv \log \frac{1}{v} \cdot \chi \left(\frac{x\sqrt{-1}}{\pi} \log \frac{1}{v} \right) \quad \left[\begin{matrix} v=0 \\ v=1 \end{matrix} \right]$$

If therefore this definite integral is finite, the theorem (A) will give correct results. A more convenient form for integration may however be obtained by the following consideration; the series

$$L \frac{x^2}{\pi^2} 1.2.3 + M \frac{x^4}{\pi^4} 1..5 + N \frac{x^6}{\pi^6} 1...7 + \&c.$$

will always be finite, if the following series

$$A + B \left(\frac{x}{\pi} \right)^2 1.2.3 + C \left(\frac{x}{\pi} \right)^4 1.2..5 + D \left(\frac{x}{\pi} \right)^6 1.2..7 + \&c.$$

is finite, because this series when prolonged to the terms

$Mx^n + Nx^{n+1} + \&c.$ will have its terms each greater than the corresponding terms of the other series. Now this series is equal to

$$\int dv f\left(\frac{x\sqrt{-1}}{\pi} \log \frac{1}{v}\right) \quad \left[\begin{matrix} v=0 \\ v=1 \end{matrix} \right] \quad (F)$$

I shall now apply some of the theorems to the investigation of the sums of series, and then explain a method which (when the equations to which it leads can be solved) will in all cases render them correct. And first let $f(\theta) = (\cos \theta)^n = (1 - \frac{\theta^2}{1.2} + \frac{\theta^4}{1.2.3.4} - \&c.)^n$ this series is capable of being expanded into another, which also proceeds according to the even powers of θ ; first let $k=0$, then comparing this with (A), we have

$$\frac{1}{2} = (\cos \theta)^n - (\cos 2\theta)^n + (\cos 3\theta)^n - \&c. \quad (3.9)$$

Let us now examine if the definite integral is finite, it is in this case.

$$\begin{aligned} \int dv \left\{ \cos \frac{\theta\sqrt{-1}}{\pi} \log \frac{1}{v} \right\}^n &= \int dv \left\{ \frac{-\frac{\theta}{\pi} \log \frac{1}{v}}{\varepsilon} + \frac{\frac{\theta}{\pi} \log \frac{1}{v}}{\varepsilon} \right\}^n = \\ &= \int dv \left(\frac{v^{\frac{\theta}{\pi}} - v^{-\frac{\theta}{\pi}}}{2} \right)^n \\ &= \frac{1}{2^n} \left\{ \frac{1}{1 - n \frac{\theta}{\pi}} + \frac{n}{1} \cdot \frac{1}{1 + (2-n) \frac{\theta}{\pi}} + \frac{n \cdot n-1}{1.2} \cdot \frac{1}{1 + (4-n) \frac{\theta}{\pi}} + \&c. \right\} \quad \left[\begin{matrix} v=0 \\ v=1 \end{matrix} \right] \end{aligned}$$

If n is a whole positive number we already know that the series (3.9) is correct; if n is a fraction the series which expresses the value of the definite integral is finite for all values of θ , except such as are contained in $\theta = -\frac{\pi}{2i-n}$ i being any whole number; if n is negative, then the series is finite for all positive values of θ ; it appears then that whatever be the value of n if θ is positive, we have

$$\frac{1}{2} = \frac{1}{(\cos \theta)^n} - \frac{1}{(\cos 2\theta)^n} + \frac{1}{(\cos 3\theta)^n} - \&c.$$

From the theorem (A) we may readily determine the value of the series

$$\frac{1}{1^{2l}(\cos \theta)^n} - \frac{1}{2^{2l}(\cos 2\theta)^n} + \frac{1}{3^{2l}(\cos 3\theta)^n} - \&c.$$

For let $f(\theta) = \frac{1}{(\cos \theta)^n} = A' + \frac{A' \theta^2}{n} + \frac{B' \theta^4}{n} + \frac{C' \theta^6}{n} + \&c.$

And the sum of the series required will be

$$S \frac{\pm 1}{i^{2k}} + A' \theta^n S \frac{\pm 1}{i^{2k-2}} + B' \theta^4 S \frac{\pm 1}{i^{2k-4}} + \&c. + \theta^{2l} K' \cdot \frac{1}{n}.$$

Which is precisely the same sum as we have already found in (2,4), except that we now find that it applies to fractional or surd values of n , as well as to whole numbers.

Let us next suppose $f(\theta) = (\tan \theta)^{2l+1} = T_1 \theta^{2l+1} + T_3 \theta^{2l+3} + T_5 \theta^{2l+5} + \&c.$ this give the series

$$\begin{aligned} \frac{(\tan \theta)^{2l+1}}{1^{2k+1}} - \frac{(\tan 2\theta)^{2l+1}}{2^{2k+1}} + \frac{(\tan 3\theta)^{2l+1}}{3^{2k+1}} - \&c. = \\ = T_1 \theta S \frac{\pm 1}{i^{2(k-l)}} + T_3 \theta^3 S \frac{\pm 1}{i^{2(k-l-1)}} + \&c. \end{aligned}$$

The definite integral in this case being

$$\begin{aligned} \int dv \left\{ \tan \frac{\theta \sqrt{-1}}{\pi} \log \frac{1}{v} \right\}^{2l+1} &= \frac{1}{(\sqrt{-1})^{2l+1}} \int dv \left\{ \frac{-1+v}{1+v^2 \frac{\theta}{\pi}} \right\}^{2l+1} \quad \left[\begin{matrix} v=0 \\ v=1 \end{matrix} \right] \\ &= \frac{1}{(\sqrt{-1})^{2l+1}} \left\{ A + \frac{B}{1+2 \frac{\theta}{\pi}} + \frac{C}{1+4 \frac{\theta}{\pi}} + \frac{D}{1+6 \frac{\theta}{\pi}} + \&c. \right\} \end{aligned}$$

Since $\frac{-1+v}{1+v^2 \frac{\theta}{\pi}} = A + Bv^{2 \frac{\theta}{\pi}} + Cv^{4 \frac{\theta}{\pi}} + \&c.$ this is always finite

if θ is positive, because it is less than $A + B + C + \&c.$ which is equal to zero.

In the former part we could only determine the value of the series

$$\frac{1}{(\sin \theta)^n} - \frac{1}{(\sin 2\theta)^n} + \frac{1}{(\sin 3\theta)^n} - \&c.$$

when n is an even number, nor will the method now employed enable us to find its value when n is odd; the reason of this is that

$$(\sin \theta)^{-n} = \left(\frac{\theta}{1} - \frac{\theta^3}{1.2.3} + \frac{\theta^5}{1.2.3.4.5} - \&c. \right)^{-n}$$

is developable in a series proceeding according to the even powers of θ only when n is an even number, if n is odd, it proceeds according to the odd powers.

The theorems contained in this second part are applicable to a very extensive class of series which have not, I believe, yet been considered. In (A) let $f(\theta) = \cos^2 \theta = \cos(\cos \theta)$ and putting $k = 0$ we have

$$\frac{\cos 1}{2} = \cos^2 \theta - \cos^2 2\theta + \cos^2 3\theta - \&c. \quad (4,1)$$

The definite integral which is the criterion of the truth of this value, is

$$\int dv \cos \frac{v^\pi + v}{2} = A + \frac{B}{1 + 2 \frac{\theta}{\pi}} + \frac{C}{1 + 4 \frac{\theta}{\pi}} + \frac{D}{1 + 6 \frac{\theta}{\pi}} + \&c. \quad \left[\begin{matrix} v=0 \\ v=1 \end{matrix} \right]$$

$$+ \frac{B}{1 - 2 \frac{\theta}{\pi}} + \frac{C}{1 - 4 \frac{\theta}{\pi}} + \frac{D}{1 - 6 \frac{\theta}{\pi}} + \&c.$$

And this is always finite unless θ is an even submultiple of π ; if we make $k = 1$ and $k = 2$, we shall have the following theorems, which are true with the same restrictions.

$$\cos 1. S \frac{\pm 1}{i^2} + \frac{\sin 1}{2} \cdot \frac{\theta^2}{2} = \frac{\cos^2 \theta}{1^2} - \frac{\cos^2 2\theta}{2^2} + \frac{\cos^2 3\theta}{3^2} - \&c. \quad (4,2)$$

$$\cos 1. S \frac{\pm 1}{i^4} + \theta^2 \frac{\sin 1}{2} S \frac{\pm 1}{i^2} - \frac{\theta^4}{2} \left(\frac{\sin 1}{1.2.3.4} + \frac{\cos 1}{8} \right) =$$

$$= \frac{\cos^2 \theta}{1^4} - \frac{\cos^2 2\theta}{2^4} + \frac{\cos^2 3\theta}{3^4} - \&c. \quad (4,3)$$

If $f(\theta) = (\cos n\theta)^n$, since this is capable of being developed

in a series proceeding according to the even powers of θ , we shall have if $k = 0$

$$\frac{(\cos^{n-1} 1)^m}{2} = (\cos^n \theta)^m - (\cos^{2n} \theta)^m + (\cos^{3n} \theta)^m - \&c. \quad (4,4)$$

and the definite integral is finite in this case, whenever θ and π are incommensurable: we may therefore in the same circumstance have the value of the series

$$\frac{(\cos^n \theta)^m}{1^{2k}} - \frac{(\cos^{2n} \theta)^m}{2^{2k}} + \frac{(\cos^{3n} \theta)^m}{3^{2k}} - \&c.$$

The theorems marked (A) and (B) in this paper correspond with that marked (12) in Mr. HERSCHEL's memoir "On various points of Analysis," printed in the Philosophical Transactions for 1814; with the first of these it coincides when n is an even number, and with the second when it is an odd one: the theorem alluded to is

$$\left\{ \frac{(-1)^{x+1}}{x^n} \cdot \frac{f(x^\theta) + (-1)^n f(1-x^\theta)}{2} \right\} = {}^0L(2) \cdot \alpha_n \theta^n + {}^1L(2) \cdot \alpha_{n-2} \theta^{n-2} + \&c.$$

$$\text{where } {}^0L = 1 - 1 + 1 - \&c. = \frac{1}{2} {}^{2n}L(2) = \frac{(2^{2x-1} - 1)\pi^{2x}}{1.2 \dots 2x} B_{2n-1}$$

Now this latter expression is the value of those series which I have expressed by $S \frac{\pm 1}{x^n}$. Both methods give the same result, and as that result is very frequently erroneous, I shall confirm the truth of the explanation I have offered, by shewing in a particular case, that if the sum of that part of the series which had been neglected as being equal to zero, is found and added to the other part, the result will no longer be erroneous: the example I shall examine is the series

$$\frac{1}{1+\theta^2} - \frac{1}{1+2^2\theta^2} + \frac{1}{1+3^2\theta^2} - \frac{1}{1+4^2\theta^2} + \&c.$$

In Mr. HERSCHEL's theorem, making $f(x) = \alpha_0 + \alpha_1 x + \alpha_2 x^2 + \&c. = \frac{1+\theta x \sqrt{-1}}{1+\theta^2 x^2}$ we have $\alpha_0 = 1$, and the equation becomes

$$S \left\{ \frac{(-1)^{x+1} \left\{ \frac{1+x\theta\sqrt{-1}}{1+x^2\theta^2} + \frac{1-x\theta\sqrt{-1}}{1+x^2\theta^2} \right\}}{2} \right\} = {}^oL(2).1$$

or $S(-1)^{x+1} \frac{1}{1+x\theta^2} = \frac{1}{2}$ or

$$\frac{1}{2} = \frac{1}{1+\theta^2} - \frac{1}{1+2^2\theta^2} + \frac{1}{1+3^2\theta^2} - \&c. \quad (4.4)$$

The same series being summed by the theorem (A) in this paper gives the same result; but then the theorem alluded to only declares this to be the true value in case a certain series is finite, which series is

$$+ \theta^2.1.2.3 + \theta^4.1.2.3.4.5 + \theta^6.1.2..7 + \&c.$$

but this series can only be finite when θ is actually equal to zero: the method which I have explained in this paper points out that the equation

$$\frac{1}{2} = \frac{1}{1+\theta^2} - \frac{1}{1+2^2\theta^2} + \frac{1}{1+3^2\theta^2} - \&c.$$

can only be depended on when $\theta = 0$, in that case it is known to be correct. I have already stated that the reason why the value of the series so found is incorrect, is that the series $-\theta^2(1^2-2^2+\&c.) + \theta^4(1^4-2^4+\&c.) - \theta^6(1^6-2^6+\&c.) + \&c.$ has been neglected because the coefficient of each term is zero. I shall now proceed to investigate the sum of this series, and shall prove that it is equal to a finite function of θ : let

$$y = c^2(1^2\varepsilon^x - 2^2\varepsilon^{2x} + \&c.) + c^4(1^4\varepsilon^x - 2^4\varepsilon^{2x} + \&c.) + \&c.$$

then y is equal to the sum of the series whose value we are seeking; if $c = \theta\sqrt{-1}$ and $x = 0$, differentiate y twice relative to x , and multiply by c^2 , and we find

$$c^2 \frac{d^2y}{dx^2} = c^4(1^4\varepsilon^x - 2^4\varepsilon^{2x} + \&c.) + c^6(1^6\varepsilon^x - 2^6\varepsilon^{2x} + \&c.) + \&c.$$

Hence the equation for determining y is

$$c^2 \frac{d^2y}{dx^2} - y = -c^2(1^2\varepsilon^x - 2^2\varepsilon^{2x} + \&c.) = -c^2 \frac{\varepsilon^x - \varepsilon^{2x}}{(1+\varepsilon^x)^2}$$

And the value of y is

$$y = \frac{c}{2} \left\{ \varepsilon^{\frac{x}{c}} \int \varepsilon^{-\frac{x}{c}} \frac{\varepsilon^{2x} - \varepsilon^x}{(1 + \varepsilon^x)^3} dx - \varepsilon^{-\frac{x}{c}} \int \varepsilon^{\frac{x}{c}} \frac{\varepsilon^{2x} - \varepsilon^x}{(1 + \varepsilon^x)^3} \right\}$$

These integrals must be taken between the limits $x = -\infty$ and $x = 0$, putting $\varepsilon^x = v$ this equation is changed into

$$y = \frac{c}{2} \left\{ v^{\frac{1}{c}} \int \frac{v-1}{(1+v)^3} v^{-\frac{1}{c}} dv - v^{-\frac{1}{c}} \int \frac{v-1}{(1+v)^3} v^{\frac{1}{c}} dv \right\}$$

where the limits of v are $v = 0$ $v = 1$, in the latter integral put $v = \frac{1}{u}$, and we have

$$y = \frac{c}{2} \left\{ v^{\frac{1}{c}} \int \frac{v-1}{(1+v)^3} v^{-\frac{1}{c}} dv + u^{\frac{1}{c}} \int \frac{u-1}{(1+u)^3} u^{-\frac{1}{c}} du \right\} \quad \left\{ \begin{matrix} v=0 & u=1 \\ v=1 & u=\infty \end{matrix} \right\}$$

but this is equal to $\frac{c}{2} v^{\frac{1}{c}} \int \frac{v-1}{(1+v)^3} v^{-\frac{1}{c}} dv$ between the limits $v = 0$ and $v = \infty$, which is equal to $-\frac{\pi}{2c \sin \frac{\pi}{c}}$ hence

$$y = -\frac{\pi}{2c \sin \frac{\pi}{c}} = c^2(1^3 - 2^3 + \&c.) + c^4(1^5 - 2^5 + \&c.) + c^6(1^7 - 2^7 + \&c.) + \&c.$$

If $c = \theta \sqrt{-1}$ we have

$$-\frac{\pi}{\theta \left\{ \frac{\pi}{\theta} - \frac{\pi}{\theta} \right\}} = -\theta^3(1^3 - 2^3 + \&c.) + \theta^4(1^5 - 2^5 + \&c.) - \theta^6(1^7 - 2^7 + \&c.) + \&c.$$

This being added to $\frac{1}{2}$ the value given by the theorem (A) produces

$$\frac{1}{2} - \frac{\pi}{\theta \left\{ \frac{\pi}{\theta} - \frac{\pi}{\theta} \right\}} = \frac{1}{1+\theta^2} - \frac{1}{1+2^2\theta^2} + \frac{1}{1+3^2\theta^2} - \frac{1}{1+4^2\theta^2} + \&c.$$

which is the same value that EULER had assigned to this series.

From the value which has been found for y , or for the series

$$c^2(1^3 - 2^3 + \&c.) + c^4(1^5 - 2^5 + \&c.) + \&c.$$

I am inclined to conclude that although the series $1^{2n} - 2^{2n} +$

$3^{2n} - \&c.$ is equal to zero for any finite value of n , yet that when n is infinite, the sum of this series is also infinite.

It was my intention to have produced from several of the series whose sums have been found in this Paper, the values of several continued products; but the length to which it has already extended will prevent me from more than merely noticing, that many very curious ones will present themselves by integrating the series whose sums have been given.

Since this Paper was written, in a conversation with M. POISSON, I mentioned one of the series which it contained, and remarked, that the principle employed led to many erroneous results; that gentleman observed, that many years before he had been led to series nearly similar, in endeavouring to integrate the equations representing the planetary motions, by means of series arranged according to some other functions of the time than the usual ones of the sines and cosines: he obligingly showed me some of his papers relating to this subject, in one of which was a series which in a particular case became the one I had mentioned; the mode of investigation by which he had arrived at these series he had however laid aside, because it rested on the sums of the diverging series $1^{2n} - 2^{2n} + 3^{2n} - \&c.$ on which he observed we cannot depend. To the same distinguished analyst I am indebted for some farther information on this subject. M. POISSON was one of the commissioners appointed by the Institute of France to examine the manuscripts which were left by LAGRANGE, amongst these was one entitled "a method of summing series," which depended on the values of the same diverging

series as those used by M. POISSON and myself; unfortunately it is very short, and its illustrious author does not appear to have resumed the subject: possibly the erroneous values which it gives for the sum of certain series might have caused him to reject it.

C. BABBAGE.

March 25, 1819.

XIX. *On the optical and physical properties of Tabasheer.* By David Brewster, LL.D. F. R. S. Lond. and Edin. In a Letter to the Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S. &c. &c. &c.

Read May 6, 1819.

MY DEAR SIR,

THE substance called Tabasheer, has been long used as a medicine in Turkey, Syria, Arabia, and Hindostan. It was first made generally known in Europe by Dr. PATRICK RUSSELL, who published in the Philosophical Transactions, for 1790, a very interesting account of its natural history, and of the process by which it seems to be formed. From his enquiries it appears, that this substance is found in the cavities of the bamboo, the *Arundo bambos* of LINNÆUS; and that it exists originally in the state of a transparent fluid, which acquires by degrees the consistency of a mucilage resembling honey, and is afterwards converted by gradual induration into a white solid, called Tabasheer. From the analysis of Mr. MACIE (now Mr. SMITHSON), it appeared to be “perfectly identical with common siliceous earth.”*

The celebrated traveller M. HUMBOLDT, discovered the same substance in the bamboos which grow to the west of Pinchincha, in South America, and a portion of what he brought to Europe in 1804, was analyzed by FOURCROY and VAUQUELIN, who found it to consist of 70 parts of silex, and 30 of potash and lime.†

* See *Philosophical Transactions* for 1791, p. 368.

† *Mémoires de l'Institut*, Tom. vi. p. 382.

About two years ago my friend, Dr. KENNEDY, received from India, a considerable quantity of tabasheer, a portion of which he presented to the Royal Society of Edinburgh. From this portion I took two or three fragments, with the view of ascertaining if it possessed any crystalline structure, but having found that it had no particular action upon polarised light, I was not led to any farther examination of its physical properties. In the course, however, of a series of experiments upon the phosphorescence of minerals, I was surprised to observe, that the tabasheer emitted a light when placed upon a hot iron, more intense than most of the phosphorescent minerals. This unexpected property induced me to resume the investigation, and having received, through the kindness of Dr. KENNEDY, an additional supply of tabasheer, I was enabled to examine it with peculiar care in all its optical and physical relations.

Among the pieces of tabasheer which I have examined, there appear to be three different kinds. The first has a milky transparency, transmitting a *yellowish*, and reflecting a *bluish white* light. It is easily broken between the fingers, and has a sort of aerial and unsubstantial texture, entirely different from any other solid substance. The second kind is more opaque, and harder than the first, having a slight degree of translucency at the edges; and the third kind is perfectly opaque, resembling a piece of stucco or chalk, or the opaque subsulphate of alumine.

If we form two parallel faces upon a piece of transparent tabasheer, by grinding it on a plate of smooth but unpolished glass, we shall be able to see objects through it with perfect distinctness, although no polish is induced upon the surfaces.

By slightly wetting the tabasheer, it loses all its transparency, and assumes the appearance of a piece of chalk; but if we immerse it in water a great quantity of gas is disengaged, the edges become more transparent than before, and a small white ball appears in the centre, which gradually diminishes till the transparency has extended itself throughout the whole mass. The same effect takes place with the second kind of tabasheer; but the third kind, though it disengages gas like the rest, never loses its chalky opacity.

The property of becoming more transparent by the expulsion of air and the absorption of water, is one which the tabasheer possesses in common with hydrophanous opal; but the faculty of retaining a considerable degree of transparency when it is dry, and its pores filled with air; and the still more extraordinary faculty of becoming quite opaque by the absorption of a small quantity of water, are possessed by no other known substance in nature, and indicate a singularity of structure which it becomes interesting to investigate.

When the pores of hydrophane are filled with air, the difference between the refractive power of the air and the solid substance is so great, that the light is scattered in every direction by refraction, and the mass is consequently white and opaque. As the tabasheer disengages a much greater quantity of air than the hydrophane, its pores must be more numerous, and therefore the transmission of the light, so as to form a perfect image, indicates either an extreme feebleness of refractive power, or some singularity in the form and construction of the pores themselves.

In order to determine this, I formed a prism of tabasheer, and upon measuring its refractive power, I found it to be

extremely low, and to vary in different specimens, as stated in the following table.

				Index of Refraction.
Transparent tabasheer from Vellore*				1.1115
Transparent tabasheer from Nagpore				1.1454
Another specimen of the same	-			1.1503
A third specimen of the same	-			1.1535
A harder and more opaque specimen,				1.1825
Water	-	-	-	1.3358
Flint glass	-	-	-	1.600
Sulphur	-	-	-	2.115
Phosphorus	-	-	-	2.224
Diamond	-	-	-	2.470

Hence it follows, that tabasheer has a lower refractive power than all other bodies, whether solid or fluid; and that it holds an intermediate place between *water* and the *gases*. This extraordinary result which, as it were, insulates tabasheer from all known substances, will enable us to give an explanation of some of its most remarkable properties.

The singular nature of this substance will appear in a still more striking point of view, by comparing its absolute refracting power with that of other bodies. The low refractive power of air, when contrasted with that of water, and of water when contrasted with that of solid bodies, obviously arises from the great difference of their specific gravities, and not from any peculiar action upon light. If we call *R* the

* This specimen, with which I was favoured by Dr. HOPE, formed part of the tabasheer sent by Dr. PATRICK RUSSELL to the Royal Society, in 1790. It was yellowish by reflected light, and so extremely tender, that I was obliged to polish it upon the softest silk.

absolute refractive power of any body, m its index of refraction, and S its specific gravity, we shall have $R = \frac{m^2 - 1}{S}$.

By means of this formula I have computed the following table, for the purpose of showing the peculiar nature of tabasheer, and the general progression in the absolute refractive powers of other bodies.

TABLE, showing the absolute refractive powers of tabasheer and other bodies.

Tabasheer	•	-	976.1*
Sulphate of barytes			3829.43
Air,	-	-	Biot 4530.

* A distinguished member of the Royal Society, whose opinion is entitled to the highest consideration, has kindly stated to me, that in estimating the absolute refractive power of tabasheer, I should have taken its specific gravity at about 0.66 in place of 2.4, which would have given an absolute refractive power not so marvellously different from that of other bodies.

My reason for retaining the original number of 976.1, is, that the result obtained by using a specific gravity of 0.66 would have been a *theoretical, and not an experimental result*. A body which sinks in water must have a density greater than unity; and I am supported by the high authority of Mr. CAVENDISH, and also by that of Mr. SMITHSON, in considering the specific gravity of tabasheer as nearly 2.412. Capillary spaces which are so large as to contain water, and even the thickest oils and varnishes, can never be regarded as forming a part of the body, though, in the present case, it is highly probable that these spaces modify the action of the solid particles in the manner which I have described.

Although the idea that tabasheer is quartz expanded till its specific gravity is reduced to about 0.66, holds out a sort of general explanation of its singular properties; yet when we consider that *hydropbane*, which is also quartz expanded till it is capable of absorbing water, is actually opaque; and when we recollect also that the refractive power of bodies does not always diminish when they expand, as is proved by the experiments of ALBERT EULER on heated glass, and by the circumstance that the point of maximum density in water is not indicated by a maximum of refractive power, its optical properties must continue to appear as marvellous as before.

Quartz, -	MALUS	5414.57
Calcareous spar,	MALUS	6423.5
Flint glass, <i>lowest</i>		7238.
Ruby -		7388.8
Brazilian topaz -		7586.7
Water, -	MALUS	7845.7
Flint glass, <i>highest</i>		8735.
Carbonate of potash		10227.
Chromate of lead		10436.
Nitre -		11962.
Muriate of soda -		12086.
Bees wax, -	MALUS	13308.1
Diamond, -	MALUS	13964.5
Sulphur - -		22000.
Phosphorus -		28857.
Hydrogen -		29964.
<hr/> - -		31862.

It appears from the preceding table, that tabasheer, which was placed between water and air, in the comparison of their indices of refraction, is now not only the lowest of all substances in its absolute refractive power, but it is so extremely low as to be separated by a great interval from them all. The very high refractive power of sulphur, phosphorus and hydrogen, and the great interval between diamond and sulphur are very remarkable, and indicate that hydrogen may enter largely into the composition of sulphur and phosphorus.

I now saturated the prism of tabasheer with *water* and with oil of cassia in succession, and I found that, in the first case, its refractive power was raised to 1.4012 higher than that of water, and in the second case to 1.6423, a little greater than

that of oil of cassia. The oil communicated to the prism a very yellow tinge, and was retained by it for a very long time.

Tabasheer readily imbibes all the volatile and fat oils, and indeed all the fluids that I have tried. The essential oils are quickly absorbed, and with the exception of oil of cassia are as quickly evaporated, while the fat oils are slowly drawn in and remain a long time in its pores; and in all these cases an opacity is produced by a partial absorption exactly as in the case of water.

When the oils or other fluids have a colour of their own, or are tinged with any colouring matter, the tabasheer exhibits a similar tint, so that it is easy to communicate to it any colour that we please. From a solution of acetate of copper, it acquires the colour of the emerald; from any of the oils coloured with anchusa root, it receives the tints of the ruby; from oil of beech nut, the colour of the chryso-beryl; from sulphuric acid, that of the pink topaz; and from malic acid, that of the Brazilian topaz. These different colours may be all discharged by exposing the tabasheer to a red heat, and thus expelling the absorbed fluid to which they owe their origin.

The opaque tabasheer, which retains its opacity when its pores are filled with water, acquires the most beautiful transparency from the absorption of oil of beech nut; and it is curious to observe a substance like chalk, and consisting apparently of a number of particles in a state of accidental aggregation, converted into a transparent mass, which the light freely penetrates in every direction. Having saturated a large piece of this kind of tabasheer with oil of beech nut, coloured with anchusa root, I laid it on a mass of lead of a lower temperature than that of the room. The oil instantly

retired from the surface of the tabasheer into its interior, and the transparent mass became opaque like a piece of red brick. Upon removing it from the lead into its former temperature, the oil returned to the surface, and the tabasheer resumed its transparency. If, on the other hand, we place it in a higher temperature than that of the room, a part of the oil will be discharged, and when it is brought back to its first temperature, it will become opaque like a piece of brick. Even when a small part of the oil remains, the transparency may be readily restored by the application of a sufficiently high degree of heat. The phenomena which have now been described admit of a satisfactory explanation from the difference between the expansion of the oil and that of the tabasheer; but the effect appears to be too great to arise from this cause, and I am rather disposed to ascribe it to a variation in the capacity of the tabasheer for the oil by a change of temperature.

In order to observe the nature of the penumbral boundary which might be supposed to separate the opaque from the transparent part, if they could be both rendered visible in the same mass, I saturated the largest piece of tabasheer that I had with the coloured oil, and having discharged a good deal of it by heat, it became of course opaque. I now held to the flame of a candle one of its extremities, which immediately became diaphanous, and the transparency gradually pervaded the opaque mass. As soon as the opacity disappeared, I allowed one extremity of it to cool; the transparency immediately disappeared at that part, and the opacity gradually advanced like a black cloud, till the whole was overshadowed by the retreat of the oil into the interior of

the mass. In both these cases the penumbra, which separated the opaque and transparent portions, had a ragged or branching appearance when seen by a microscope, as if the oil had been shooting into crystals during the progress of the opacity, and as if these crystals had been dissolving during its retreat.

Upon examining the appearance of the tabasheer when it had the colour of red brick, after the discharge of a portion of its oil, I was surprised to observe a beautiful veined structure, exactly like that of the agate, the veins being sometimes parallel, sometimes inflected, and sometimes curved. In some specimens the veins were alternately opaque and translucent, and in others red and white, as if one set of strata had a greater capacity for the oil than the rest. This effect is almost universal; but as soon as the oil is completely discharged, the veined structure entirely disappears, and the whole mass assumes the homogeneous appearance of chalk.

In order to observe the circumstances under which the chalky tabasheer became transparent by the absorption of oils, I cut four plates out of the same piece, and immersed them separately in oil of cassia, alcohol, water, and oil of beech nut. The plates that had absorbed the three first of these fluids remained quite opaque, but the plate that was placed in the oil of beech nut gradually acquired a translucency by the rapid extrication of air. After a certain time it appeared to be covered with scratches and small opaque portions; but these appearances, which arose from remaining vesicles of air, vanished by degrees. By the application of a microscope, I observed the air form itself into globules in the interior of the tabasheer, which slowly advanced to the edge of it, and at last escaped into the oil. After a lapse of

nearly two hours the greater part of the air was extricated, and the expulsion of the remainder was quickly effected by a gentle heat; but the transparency was of that imperfect kind, which results from the union of two bodies of different refractive powers. By increasing the heat, the tabasheer became more transparent, and at a certain temperature it could scarcely be seen in the oil in which it was placed. When the heat was still farther augmented, it became more and more opaque, and a corresponding opacity was induced by cooling it down as much below the temperature of maximum transparency.

When pure tabasheer is boiled for any length of time, or is brought to a red or a white heat, it suffers no change either in its colour or in its optical and physical properties: if we wrap it, however, in a piece of paper, and set the paper on fire, the tabasheer becomes either black, or brownish black, and the black colour increases in depth by the repetition of the experiment. When immersed in water, it disengages the included air, but with less rapidity than before; and when it is broken and pounded, its fracture and its powder are black.

If the blackened tabasheer is brought to a red heat, it is restored to its primitive whiteness, and resumes all its former properties; but if the heat is considerably below redness, some specimens acquire a slight transparency, and a dark slaty blue colour, shading in some places into whiteness. When slightly wetted in this state, it becomes chalky white; with a greater portion of water it becomes black; and with a still greater portion, it becomes again transparent. If we break a piece of tabasheer, a few days after it has received a deep

black tint, we shall find that it has often a fine ash grey colour, which becomes deeply black when wetted, and afterwards resumes its primitive tint when dry.

As the blackness communicated to the tabasheer is not produced by heat alone, or by any particular method of cooling, it cannot arise from any mechanical change, similar to that which THENARD observed in the cooling of melted phosphorus; and there is reason to believe that it does not arise from the absorption of any sooty matter thrown off during the combustion of the paper, as the blackened tabasheer almost always recovers a certain degree of whiteness, and imbibes water almost as freely as in its original state. With the view of discovering if the blackness was owing to any vegetable matter in the tabasheer, I repeated the operation of blackening it and restoring its colour by heat about 50 times; but after all these operations, it became black as readily as at first. The specimen that had undergone these changes had increased in hardness and lustre, and had the appearance of the finest Indian ink. Upon breaking it in two, the fracture was perfectly black, but assumed a dark blue colour by exposure to the air; and upon putting a drop of pure water upon the blue fracture, it was instantly converted into a deep and glossy black.

When the pure tabasheer was exposed to a white heat for several hours, and was then burned in paper, it received its black colour as before. When it was held in the flame of alcohol or of carburetted hydrogen, it was merely stained in a slight degree, which was probably owing to the intensity of the heat, which may have discharged the black tint as soon as it was formed.

I was now anxious to see the effect produced upon tabasheer by the absorption of iodine gas, and for this purpose I took several opaque and transparent fragments, and having saturated some of them with moisture, and left others dry, I placed them in different glass tubes, which were hermetically sealed after a portion of iodine had been introduced. Previous to the application of heat the tabasheer assumed a yellow tinge, which deepened into a pale orange, and a veined structure appeared in one of the fragments. When the iodine was converted into gas by heat, the colour of the fragments grew more and more red; the transparent pieces were like garnets, and the opaque ones like fragments of red brick; and after standing two or three days the opaque pieces became perfectly transparent. The iodine vapour, therefore, seems to have taken the place of the water in the wetted fragments, and of the common air in the dry ones; and it appears to have retained its gaseous form within the tabasheer, when the external gas had returned to the solid state. Upon taking the tabasheer out of the glass tubes, the iodine was slowly expelled, a yellow tinge appeared even after 30 hours exposure to the air, and it was not entirely removed by immersing the fragments in water.

The difference in the properties of the opaque and the transparent tabasheer, rendered it desirable to have accurate measures of their specific gravities, and I have been fortunate enough to procure them to a great degree of exactness, through the kindness of my friend, Mr. JAMES JARDINE, who obtained for me the following results.

	Grains.
No. I. Seven pieces of opaque tabasheer,	
weighed in air. - -	6 65

The same pieces when thoroughly soaked in water, weighed in air - 14,10

The same weighed in water at the temperature of 52° - - 3,42

Hence the specific gravity of the parcel when dry, is 2.059

And the specific gravity of the parcel when wet, is 1.320

	Grains
No. II. Several small fragments of transparent tabasheer, weighed in air -	1,23

The same pieces when thoroughly soaked in water, weighed in air - 2,54

The same weighed in water at the temperature of 52° - - 0,72

Hence the specific gravity of the parcel when dry, is 2,412

And the specific gravity of the parcel when wet, is 1,396

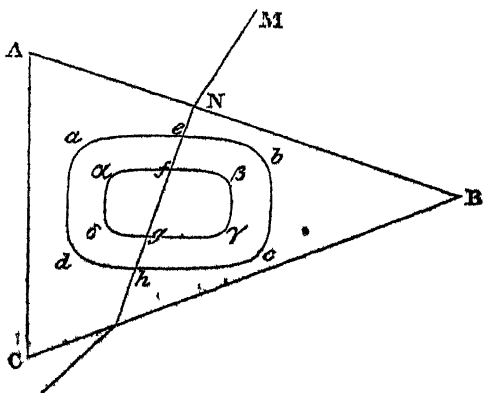
Mr. MACIE found the specific gravity of a parcel of opaque and transparent tabasheer to be 2,188; and Mr. CAVENDISH, having tried the same parcel, found it to be 2,169. The mean of Mr. JARDINE's results is 2,235, which exceeds the measures of MACIE and CAVENDISH, because the opaque fragments in their parcels must have been more numerous than the transparent ones, in consequence of the rarity of the latter.

It appears from the preceding results, that in both kinds of tabasheer the quantity of water imbibed exceeds in weight that of the tabasheer itself; and that in the opaque kinds, the space occupied by the pores is to the space occupied by the tabasheer, as 2,307 to 1; while in the transparent kinds, it is 2,5656 to 1. This result indicates a very remarkable

degree of porosity ; and as it makes the pore more extensive in the transparent than in the opaque kind, contrary to what we should expect from their specific gravities, it seems to follow, that the water was not capable of insinuating itself into all the pores of the opaque tabasheer. This conclusion is rendered more probable, when we consider the extreme difficulty with which the oil of beech nut displaces the last portions of included air ; and it affords a very plausible explanation of the fact, that the chalky tabasheer cannot be rendered transparent by the absorption of water.

We are now prepared by the preceding observations, for investigating the cause of the remarkable paradox exhibited by the transparent tabasheer, in becoming perfectly opaque and white, by absorbing a small quantity of water, and perfectly transparent when that quantity is increased. As this effect takes place indiscriminately with all fluids, it cannot be the result of any chemical action, and therefore its cause must be sought for in the changes which the light suffers in traversing the vacuities of the tabasheer.

Let ABC be a prism of this substance, and *abcd* one of its pores highly magnified. We know that this pore is filled with air ; and that when a ray of light MN enters the separating surface *ab* at *e*, and quits it at *h*, it suffers so little refraction, and is therefore so little scattered, that the tabasheer appears transparent, and allows us to see objects



distinctly through it. This effect, which could not take place in any other porous substance, arises from the small difference between the refractive power of air and tabasheer. Let us now suppose that a small quantity of water is introduced into the pore $abcd$, so as not to fill it, but merely to line its circumference with a film contained between $abcd$ and $\alpha\beta\gamma\delta$. Then the light which was formerly scattered by the slight refraction at e and h in passing from tabasheer into air, will now be a little less scattered at these points, since it passes from tabasheer into water, where the difference of refractive power is less; but in passing from the film of water into the air at f , and in entering the water again at g , the scattering of the rays will be very considerable, from the great difference in the refractive powers of air and water. In passing through every pore, therefore, the light is refracted, and consequently scattered no less than four times; and hence the piece of tabasheer must appear to be opaque. If we now saturate it with water, the pore $abcd$ will be completely filled; the two great refractions which took place at f and g , will no longer exist; and the light will suffer only a slight refraction at e and h , by which it will be less scattered than when the tabasheer was dry. Hence it follows, that when the tabasheer is saturated with water, it ought to transmit the incident light freely, and to be more transparent than when it is quite dry, a result which is perfectly conformable to observation.

The very singular anomaly presented by the chalky tabasheer in becoming transparent by means of oil of beech nut, and not by means of oil of cassia or water, and the additional transparency communicated to the transparent

fragments by immersion in oil, if they do not arise from the existence of minute pores, which admit the oil and keep out the water, must be ascribed to a very considerable refractive density in the solid parts of the substance. The experiments made with the oil at different temperatures, indeed, seem to prove that the refractive power of the solid parts of the tabasheer is equal to about 1,500, or that of oil of beech nut raised to the temperature which produces the maximum transparency; but as the refractive power of a prism of tabasheer is greatly inferior to that of water, we shall now proceed to consider how these apparently opposite results can be reconciled, and what inferences can be drawn from them.

When alcohol is poured into water in a glass vessel, a scattering of the transmitted light is immediately visible, in consequence of their imperfect mixture, and of the difference of their refractive powers; but in a short time the union of the fluids is so complete, that the light is transmitted as freely as through either of them separately. Chemistry does not inform us how the particles of alcohol and of water are combined so as to produce this effect; but we know that the refractive power of the compound is intermediate between that of the two ingredients; and hence it is certain, that the refraction of the incident light is produced by their joint action. If we increase the quantity of water successively, the particles of alcohol will be thrown to a greater and greater distance, and the refractive power of the compound will be proportionally diminished. Let us now suppose that all the aqueous particles are extinguished, and that their place is occupied with a less refractive fluid, such as air; then it is obvious that the

refractive power of the compound will be inferior to that of water, and will approach to that of air, in proportion to the quantity of air united with the alcohol.

This hypothetical combination of air and alcohol, represents exactly what I conceive to be the condition of the dry and transparent tabasheer. The refractive power of the solid parts is not far from 1,500; but the substance is so formed, that its particles are kept at a great distance by means of vacuities filled with air, and arranged in such a manner, that the light passes from the particles of tabasheer into the particles of air without suffering refraction, in the same manner as it passes from the particles of alcohol to the particles of water, when these fluids are chemically united. That there is not a chemical union between the air and the tabasheer, is certain, because the air may be displaced, and the transparency preserved in *vacuo*; and hence we may draw the important inference, of which we have no other example in physics, that the tabasheer and its included air exercise a joint action upon light, in the same manner as if they were in a state of chemical union. I have the honour to be, &c. &c. &c.

DAVID BREWSTER.

Edinburgh, March 2, 1819.

XX. *An Account of a Membrane in the Eye, now first described.*
 By Arthur Jacob, M. D. Member of the Royal College of
 Surgeons in Ireland, Demonstrator of Anatomy and Lecturer
 on Diseases of the Eye in the University of Dublin. Commu-
 nicated by James Macartney, M. D. F. R. S.

Read July 1, 1819.

ANATOMISTS describe the retina as consisting of two portions, the medullary expansion of the nerve, and a membranous or vascular layer. The former externally, next to the choroid coat, and the latter internally, next to the vitreous humor.* All however, except ALBINUS and some of his disciples, agree, that the nervous layer cannot be separated so as to present the appearance of a distinct membrane, though it may be scraped off, leaving the vascular layer perfect. That the medullary expansion of the optic nerve is supported by a vascular layer, does not I think admit of doubt; but it does not appear that ALBINUS was right in supposing that the nervous layer can be separated in form of a distinct membrane, though shreds of a considerable size may be detached, especially if hardened by acid or spirit.

Exclusive of these two layers, I find that the retina is covered on its external surface by a delicate transparent

* RUYSCH. Epist. Anat. Prob. xiii. ALBINUS, Annot. Acad. lib. iii. cap. xiv. HALLER, Elem. Phys. T. v. lib. xvi. sect. 2. ZINN. Descrip. Anat. Oculi. cap. iii. sect. iii. 'SARATIER, BOYER, CHARLES BELL, CUVIER, &c. &c.

membrane, united to it by cellular substance and vessels. This structure, not hitherto noticed by anatomists, I first observed in the spring of the last year, and have since so frequently demonstrated, as to leave no doubt on my mind of its existence as a distinct and perfect membrane, apparently of the same nature as that which lines serous cavities. I cannot describe it better, than by detailing the method to be adopted for examining and displaying it. Having procured a human eye, within forty-eight hours after death, a thread should be passed through the layers of the cornea, by which the eye may be secured under water, by attaching it to a piece of wax, previously fastened to the bottom of the vessel, the posterior half of the sclerotic having been first removed. With a pair of dissecting forceps in each hand, the choroid coat should be gently torn open and turned down. If the exposed surface be now carefully examined, an experienced eye may perceive, that this is not the appearance usually presented by the retina; instead of the blue-white reticulated surface of that membrane, a uniform villous structure, more or less tinged by the black pigment, presents itself. If the extremity of the ivory handle of a dissecting knife be pushed against this surface, a breach is made in it, and a membrane of great delicacy may be separated and turned down in folds over the choroid coat, presenting the most beautiful specimen of a delicate tissue which the human body affords. If a small opening be made in the membrane, and the blunt end of a probe introduced beneath, it may be separated throughout, without being turned down, remaining loose over the retina; in which state if a small particle of paper or globule of air be introduced under it, it is raised so as to be seen against the

light, and is thus displayed to great advantage ; or it is sometimes so strong as to support small globules of quicksilver dropped between it and the retina, which renders its membranous nature still more evident. If a few drops of acid be added to the water after the membrane has been separated, it becomes opaque and much firmer, and may thus be preserved for several days, even without being immersed in spirit.

That it is not the nervous layer which I detach, is proved by the most superficial examination ; first, because it is impossible to separate that part of the retina, so as to present the appearance I mention ;* and, secondly, because I leave the retina uninjured, and presenting the appearance described by anatomists, especially the yellow spot of SOEEMMERRING, which is never seen to advantage until this membrane be removed : and hence it is that that conformation, as well as the fibrous structure of the retina in some animals, becomes better marked from remaining some time in water, by which the membrane I speak of is detached.

The extent and connections of this membrane are sufficiently explained by saying, that it covers the retina from the optic nerve to the ciliary processes. To enter into farther investigation on this subject, would lead to a discussion respecting the structure of the optic nerve, and the termination of the retina anteriorly, to which it is my intention to return at a future period.

The appearance of this part I find to vary in the different classes of animals and in man, according to age and other circumstances. In the foetus of nine months it is exceed-

* See HALLER, ZINN, &c. loc. cit.

ingly delicate, and with difficulty displayed. In youth it is transparent, and scarcely tinged by the black pigment. In the adult it is firmer, and more deeply stained by the pigment, which sometimes adheres to it so closely as to colour it almost as deeply as the choroid coat itself; and to those who have seen it in this state, it must appear extraordinary that it should not have been before observed. In one subject, aged fifty, it possessed so great a degree of strength as to allow me to pass a probe under it, and thus convey the vitreous humor covered by it and the retina from one side of the basin to the other; and in a younger subject I have seen it partially separated from the retina by an effused fluid. In the sheep, ox, horse, or any other individual of the class mammalia which I have had an opportunity of examining, it presents the same character as in man; but is not so much tinged by the black pigment, adheres more firmly to the retina, is more uniform in its structure, and presents a more elegant appearance when turned down over the black choroid coat. In the bird, it presents a rich yellow brown tint, and when raised, the blue retina presents itself beneath; in animals of this class, however, it is difficult to separate it to any extent, though I can detach it in small portions. In fishes, the structure of this membrane is peculiar and curious. It has been already described as the medullary layer of the retina by HALLER and CUVIER,* but I think incorrectly, as it does not present any of the characters of nervous structure, and the retina is found perfect beneath it. If the sclerotic coat be removed behind, with the choroid coat and gland so

* *Element. Phys. T. v. lib. xvi. sect. ii.* CUVIER *Leçons d'Anat. Comp. T. ii. p. 419.*

called, the black pigment is found resting upon, and attached to, a soft friable thick fleecy structure, which can only be detached in small portions, as it breaks when turned down in large quantity. Or if the cornea and iris be removed anteriorly, and the vitreous humor and lens withdrawn, the retina may be pulled from the membrane, which remains attached to the choroid coat, its inner surface not tinged by the black pigment, but presenting a clear white, not unaptly compared by HALLER to snow.

Besides being connected to the retina, I find that the membrane is also attached to the choroid coat, apparently by fine cellular substance and vessels; but its connection with the retina being stronger, it generally remains attached to that membrane, though small portions are sometimes pulled off with the choroid coat. From this fact I think it follows, that the accounts hitherto given of the anatomy of these parts, are incorrect. The best anatomists* describe the external surface of the retina as being merely in contact with the choroid coat, as the internal with the vitreous humor, but both totally unconnected by cellular membrane, or vessels, and even having a fluid secreted between them: some indeed speak loosely and generally of vessels passing from the choroid to the retina; but obviously not from actual observation, as I believe no one has ever seen vessels passing from the one membrane to the other. My observations lead me to conclude, that wherever the different parts of the eye are in

* See HALLER, Elem. Phys. T. v. lib. xvi. sect. ii. ZINN, cap. ii. sect. i. § ii. BOYER, Anat. T. iv. p. 113. SABATIER, T. ii. p. 70. BICHAT, Anat. Descr. T. ii. p. 447. COVIER, Leçons d'Anat. Comp. T. ii. p. 418. CHARLES BELL, Anat. vol. iii. p. 53. RIBES, Mém. de la Soc. Med. d'Emulation, T. viii. p. 633.

contact, they are connected to each other by cellular substance, and, consequently, by vessels; for I consider the failure of injections no proof of the want of vascularity in transparent and delicate parts, though some anatomists lay it down as a criterion. Undoubtedly the connection between these parts is exceedingly delicate, and, hence, is destroyed by the common method of examining this organ; but I think it is proved in the following way. I have before me the eye of a sheep killed this day, the cornea secured to a piece of wax fastened under water, and the posterior half of the sclerotic coat carefully removed. I thrust the point of the blade of a pair of sharp scissors through the choroid coat into the vitreous humor, to the depth of about an eighth of an inch, and divide all, so as to insulate a square portion of each membrane, leaving the edges free, and consequently no connection except by surface; yet the choroid does not recede from the membrane I describe, the membrane from the retina, nor the retina from the vitreous humor. I take the end of the portion of choroid in the forceps, turn it half down, and pass a pin through the edge, the weight of which is insufficient to pull it from its connection. I separate the membrane in like manner, but the retina I can scarcely detach from the vitreous humor, so strong is the connection. The same fact may be ascertained by making a transverse vertical section of the eye, removing the vitreous humor from the posterior segment, and taking the retina in the forceps, pulling it gently from the choroid, when it will appear beyond a doubt that there is a connection between them.

Let us contrast this account of the matter with the common one. The retina, a membrane of such delicacy, is

described as being extended between the vitreous humor and choroid, from the optic nerve to the ciliary processes, being merely laid between them, without any connection, and the medullary fibres in contact with a coloured mucus retained in its situation by its consistence alone. This account is totally at variance with the general laws of the animal economy: in no instance have we parts, so dissimilar in nature, in actual contact: wherever contact without connection exists, each surface is covered by a membrane, from which a fluid is secreted; and wherever parts are united, it is by the medium of cellular membrane, of which serous membrane may be considered as a modification. If the retina be merely in contact with the vitreous humor and choroid, we argue from analogy, that a cavity lined by serous membrane exists both on its internal and external surface; but this is not the fact. In the eye a distinction of parts was necessary, but to accomplish this a serous membrane was not required; it is only demanded where great precision in the motion of parts was indispensable, as in the head, thorax, and abdomen; a single membrane, with the interposition of cellular substance, answers the purpose here. By this explanation we surmount another difficulty, the unphilosophical idea of the colouring matter being laid on the choroid, and retained in its situation by its viscosity, is discarded; as it follows, if this account be correct, that it is secreted into the interstices of fine cellular membrane here, as it is upon the ciliary processes, back of the iris, and pecten, under the conjunctiva, round the cornea, and in the edge of the membrana nictitans and sheath of the optic nerve in many animals. Dissections are recorded where fluids have been found collected between the choroid and

retina, by which the structure of the latter membrane was destroyed ; the explanation here given is as sufficient to account for the existence of this fluid, as that which attributes it to the increased secretion of a serous membrane.

I take this opportunity of describing the method I adopt for examining and displaying these and other delicate parts, a method, which though simple, will, I expect, prove an important improvement in the means of scrutinizing the structure of animal and vegetable bodies. I procure a hollow sphere of glass from two to three inches in diameter, about one fourth of which is cut off at the part where it is open, and the edges ground down, so as to fit accurately upon a piece of plate glass, the surface of which is also ground ; the object to be examined is attached to a piece of wax fastened upon the plate of glass and immersed in a basin of water, with the cut sphere, which is inverted over it, of course full of water, and the whole withdrawn from the basin. The part may thus be examined under the most favourable circumstances ; it floats in water, the only method by which delicate parts can be unfolded and displayed : the globular form of the vessel answers the purpose of a lens of considerable power and perfection, at the same time that it admits light in any quantity or direction to illuminate the object ; and, what is of the utmost importance, a preparation of the greatest delicacy may thus be handed round a class in safety.

XXI. *A new method of solving numerical equations of all orders, by continuous approximation.** By W. G. Horner, Esq.
Communicated by Davies Gilbert, Esq. F. R. S.

Read July 1, 1819.

1. **T**HE process which it is the object of this Essay to establish, being nothing else than the leading theorem in the Calculus of Derivations, presented under a new aspect, may be regarded as a universal instrument of calculation, extending to the composition as well as analysis of functions of every kind. But it comes into most useful application in the numerical solution of equations.

2. ARBOGAST's developement of

$$\phi (\alpha + \beta x + \gamma x^2 + \delta x^3 + \epsilon x^4 + \dots)$$

(See *Calc. des Der.* § 33) supposes all the coefficients within the parenthesis to be known previously to the operation of ϕ . To the important cases in which the discovery of γ , δ , &c. depends on the previous developement of the partial functions

$$\phi (\alpha + \beta x), \phi (\alpha + \beta x + \gamma x^2), \&c.$$

* The only object proposed by the author in offering this Essay to the acceptance of the Royal Society, for admission into the Philosophical Transactions, is to secure beyond the hazard of controversy, an Englishman's property in a useful discovery. Useful he may certainly be allowed to call it, though the produce of a purely mathematical speculation; for of all the investigations of pure mathematics, the subject of *approximation* is that which comes most directly, and most frequently into contact with the practical wants of the calculator.

How far the manner in which he has been so fortunate as to contemplate it has conducted, by the result, to satisfy those wants, it is not for him to determine; but his belief is, that both Arithmetic and Algebra have received some degree of improvement, and a more intimate union. The abruptness of transition has been broken down into a gentle and uniform activity.

it is totally inapplicable. A theorem which should meet this deficiency, without sacrificing the great facilitating principle of attaching the functional symbols to α alone, does not appear to have engaged the attention of mathematicians, in any degree proportionate to the utility of the research. This desideratum it has been my object to supply. The train of considerations pursued is sufficiently simple ; and as they have been regulated by a particular regard to the genius of arithmetic, and have been carried to the utmost extent, the result seems to possess all the harmony and simplicity that can be desired ; and to unite to continuity and perfect accuracy, a degree of facility superior even to that of the best popular methods.

Investigation of the Method.

3. In the general equation

$$\phi x = 0$$

I assume $x = R + r + r' + r'' + \dots$

and preserve the binomial and continuous character of the operations, by making successively

$$\begin{aligned} x &= R + z = R + r + z' \\ &= R' + z' = R' + r' + z'' \\ &= R'' + z'' = \&c. \end{aligned}$$

Where R^* represents the whole portion of x which has already been subjected to ϕ , and $z = r + z'$ the portion still excluded ; but of which the part r is immediately ready for use, and is to be transferred from the side of z to that of R , so as to change ϕR^* to ϕR^* without suspending the corrective process.

4. By TAYLOR's theorem, expressed in the more convenient manner of ARBOGAST, we have

$$\begin{aligned} \phi x &= \phi (R + z) = \\ \phi R + D\phi R . z + D^2 \phi R . z^2 + D^3 \phi R . z^3 + \dots \end{aligned}$$

Where by $D^n \phi R$ is to be understood $\frac{d^n \phi R}{1.2 \dots n . dR^n}$, viz. the n^{th} derivate with its proper denominator; or, that function which ARBOGAST calls the *derivée divisée*, and distinguishes by a c subscribed. Having no occasion to refer to any other form of the derivative functions, I drop the distinctive symbol for the sake of convenience. Occasionally these derivees will be represented by a, b, c , &c.

5. Supposing ϕR and its derivees to be known, the mode of valuing $\phi R'$ or $\phi(R+r)$ is obvious. We have only to say in the manner of LAGRANGE, when preparing to develope his Theory of Functions,

$$\begin{aligned}\phi R' &= \phi R + Ar \\ A &= D\phi R + Br \\ B &= D^2\phi R + Cr \\ C &= D^3\phi R + Dr\end{aligned}$$

$$\begin{aligned}V &= D^{n-2} \phi R + Ur \\ U &= D^{n-1} \phi R + r \quad . \quad . \quad . \quad . \quad [I.] \end{aligned}$$

Taking these operations in reverse order, we ascend with rapidity to the value of $\phi(R+r)$ or $\phi R'$.

6. The next point is, to apply a similar principle to discover the value of $\phi(R+r+r') = \phi(R'+r') = \phi R''$. We here have

$$\begin{aligned}\phi R'' &= \phi R' + A' r' \\ A' &= D\phi R' + B' r' \\ B' &= D^2\phi R' + C' r' \\ C' &= D^3\phi R' + D' r' \\ V' &= D^{n-1} \phi R' + U' r' \\ U' &= D^{n-1} \phi R' + r'\end{aligned}$$

But the former operation determined $\phi R'$ only, without giving the value of any of the derived functions. The very simple

scale of known quantities, therefore, by which we advance so rapidly in the first process, fails in those which follow.

7. Still we can reduce these formulæ to known terms; for since we have in general

$$D^r D^s \phi \alpha = \frac{r+1}{1} \cdot \frac{r+2}{2} \dots \frac{r+s}{s} D^{r+s} \phi \alpha$$

(See ARBOGAST, § 137); by applying a similar reduction to the successive terms in the developement of $D^m \phi R' = D^m \phi (R + r)$, we obtain*

$$\begin{aligned} D^m \phi R' = D^m \phi R + \frac{m+1}{1} D^{m+1} \phi R \cdot r + \frac{m+1}{1} \cdot \frac{m+2}{2} D^{m+2} \phi R \cdot r^2 \\ + \frac{m+1}{1} \dots \frac{m+3}{3} D^{m+3} \phi R \cdot r^3 + \&c. \end{aligned}$$

And it is manifest that this expression may be reduced to a form somewhat more simple, and at the same time be accommodated to our principle of successive derivation, by introducing the letters A, B, C, &c. instead of the functional expressions.

8. As a general example, let

$$M = D^m \phi R + Nr$$

$$N = D^{m+1} \phi R + Pr$$

$$P = D^{m+2} \phi R + Qr$$

* This theorem, of which that in Art. 4 is a particular case [$m=0$], has been long in use under a more or less restricted enunciation, in aid of the transformation of equations. HALLEY's *Speculum Analyticum*, NEWTON's limiting equations, and the formulæ in SIMPSON's *Algebra* (ed. 5, p. 166, circa fin.) are instances. In a form still more circumscribed [$r=1, R=0, 1, 2, \&c.$] it constitutes the *Nouvelle Méthode* of BUDAN; which has been deservedly characterized by LAGRANGE as simple and elegant. To a purpose which will be noticed hereafter, it applies very happily; but regarded as an instrument of approximation, its extremely slow operation renders it perfectly nugatory: and as LEGENDRE justly reported, and these remarks prove, it has not the merit of originality.

represent any successive steps in the series in Art. 5; then are

$$D^m \phi R = M - Nr$$

$$D^{m+1} \phi R = N - Pr$$

$$D^{m+2} \phi R = P - Qr$$

And by substituting these equivalents in the developement just enounced, it becomes

$$D^m \phi R' = M + m Nr + \frac{m \cdot m + 1}{1 \cdot 2} Pr^2 + \frac{m \cdot \dots \cdot m + 2}{1 \cdot \dots \cdot 3} Qr^3 + \&c.$$

9. With this advantage, we may now return to the process of Art. 6, which becomes

$$\phi R'' = \phi R' + A' r'$$

$$A' = (A + Br + Cr^2 + Dr^3 + Er^4 + \&c.) + B' r'$$

$$B' = (B + 2 Cr + 3 Dr^2 + 4 Er^3 + \&c.) + C' r'$$

$$C' = (C + 3 Dr + 6 Er^2 + \&c.) + D' r'$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$V' = (V + \frac{n-2}{1} Ur + \frac{n-2 \cdot n-1}{1 \cdot 2} r^2) + U' r'$$

$$U' = (U + \frac{n-1}{1} r) + r' \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad [II.]$$

Taking these operations in reverse order as before, by determining $U', V' \dots C', B', A'$, we ascend to the value of $\phi R''$.

10. In this theorem, the principle of successive derivation already discovers all its efficacy; for it is obvious that the next functions $U'', V'' \dots C'', B'', A'', \phi R'''$, flow from the substitution of $A', B', C', \dots V', U', \phi R'', r', r'$, for $A, B, C \dots V, U, \phi R', r, r'$, in these formulæ; and from these $U''', V''', \&c.$; and so on to any desirable extent. In this respect, Theorem II, algebraically considered, perfectly answers the end proposed in Art. 2.

11. We perceive also, that some advance has been made toward arithmetical facility; for all the figurate coefficients

here employed are lower by one order than those which naturally occur in transforming an equation of the n^{th} degree. But it is much to be wished, that these coefficients could be entirely dispensed with. Were this object effected, no multipliers would remain, except the successive corrections of the root, and the operations would thus arrange themselves, in point of simplicity, in the same class as those of division and the square root.

12. Nor will this end appear unattainable, if we recur to the known properties of figurate numbers; which present to our view, as *equivalent to the n^{th} term of the m^{th} series*:

1. *The difference of the n^{th} and $n-1^{\text{th}}$ term of the $m+1^{\text{th}}$ series.*

2. *The sum of the first n terms of the $m-1^{\text{th}}$ series.*

3. *The sum of the n^{th} term of the $m-1^{\text{th}}$, and the $n-1^{\text{th}}$ term of the m^{th} series.*

The dépression already attained has resulted from the first of these properties, and a slight effort of reflection will convince us that the second may immediately be called to our aid.

13. For this purpose, let the results of Art. 9 be expressed by the following notation:

$$\phi R'' = \phi R' + A' r'$$

$$A' = A_1 + B' r'$$

$$B' = B_2 + C' r'$$

$$C' = C_3 + D' r'$$

$$V' = V_{n-2} + U' r'$$

$$U' = U_{n-1} + r'$$

the exponents subjoined to any letter indicating the degree

of the figurate coefficients in that formula of the theorem, of which such letter is the first term.

14. Although this statement appears only to have returned to us the conditions of Art. 6, with all their disadvantages, and to have merely substituted

A_1 for $D\phi R'$ or a'

B_2 for $D^2\phi R'$ or b'

C_3 for $D^3\phi R'$ or c'

&c. yet, by means of the property just alluded to, the essential data A, B, C, &c. which have disappeared, will again be extricated. For the developement of $D^m\phi R'$, found in Art. 8, undergoes thereby the following analysis :

$$M + m Nr + \frac{m \cdot m + 1}{1 \cdot 2} Pr^2 + \frac{m \cdot m + 1 \cdot m + 2}{1 \cdot 2 \cdot 3} Qr^3 +$$

$$= M + Nr + Pr^2 + Qr^3 +$$

$$+ Nr + 2 Pr^2 + 3 Qr^3 +$$

$$+ Nr + 3 Pr^2 + 6 Qr^3 +$$

$$+ Nr + m Pr^2 + \frac{m \cdot m + 1}{1 \cdot 2} Qr^3 + \dots\dots$$

which equivalence will be thus expressed:

$$M_m = M + N_1 r + N_2 r^2 + N_3 r^3 + \dots\dots\dots + N_m r^m$$

Returning therefore once more to our theorem, we now have

$$\phi R'' = \phi R' + A' r'$$

$$A = (A + B_1 r) + B' r'$$

$$B' = (B + C_1 r + C_2 r^2) + C' r'$$

$$C' = (C + D_1 r + D_2 r^2 + D_3 r^3) + D' r'$$

$$\dots\dots\dots$$

$$V' = (V + U_1 r + U_2 r^2 + U_3 r^3 + \dots\dots\dots U_{n-1} r^{n-1}) + U' r'$$

$$U' = (U + \overline{n-1} . r) + r'$$

15. This theorem employs exactly the same total number of addends as Theorem II, but with the important improvement, that the number of addends to each derivee is inversely as their magnitude, contrary to what happened before. Figurate multipliers are also excluded. And it is easy to convince ourselves that no embarrassment will arise from the newly introduced functions. For if we expand any of the addends $N_k r$ in the general formula equivalent to M_m , and analyze it by means of the *third property* of figurate series, we shall find

$$M_k r = N_{k-1} r + P_k r r.$$

And since we take the scale in our Theorem in a reverse or ascending order, this formula merely instructs us to multiply an addend already determined by r , and to add the product to another known addend; and if we trace its effect through all the descending scale, to the first operations, we observe that the addends to the last derivee, from which the work begins, are simply r repeated $n-1$ times.

16. Because $N_0 = N$, the addend exterior to the parenthesis, might for the sake of uniformity be written $N_0' r'$. The harmony of the whole scheme would then be more completely displayed. To render the simplicity of it equally perfect, we may reflect that as the factors $r, r', \&c.$ are engaged in no other manner than has just been stated, viz. in effecting the subordinate derivations, their appearance among the principal ones is superfluous, and tends to create embarrassment. Assume therefore

$${}_k N = N_k r,$$

and we have

$$\phi R'' = \phi R' + {}_0A'$$

$$A' = (A + {}_1B) + {}_0B'$$

$$B' = (B + {}_1C + {}_2C) + {}_0C'$$

$$C' = (C + {}_1D + {}_2D + {}_3D) + {}_0D'$$

$$V' = (V + {}_1U + {}_2U + {}_3U + \dots + {}_{n-2}U) + {}_0U'$$

$$U' = (U + {}_{n-1}r) + r' \quad \text{[III]}$$

the subordinate derivations being understood.

17. The Theorems hitherto give only the synthesis of ϕx , when $x = R + r + r' + \&c.$ is known. To adapt them to the inverse or analytical process, we have only to subtract each side of the first equation from the value of ϕx ; then assuming $\phi x - \phi R = \Delta$, we have

$$\Delta' = \Delta - {}_0A$$

$$A = a + {}_0B$$

&c. as in Theorem I.

$$\Delta'' = \Delta' - {}_0A'$$

$$A' = (A + {}_1B) + {}_0B'$$

&c. as in Theorem II. or III.

The successive invention of $R, r, r', \&c.$ will be explained among the numerical details. In the mean time, let it be observed that these results equally apply to the popular formula $\phi x = \text{constant}$, as to $\phi x = 0$.

18. I shall close this investigation, by exhibiting the whole chain of derivation in a tabular form. The calculator will then perceive, that the algebraic composition of the addends no longer requires his attention. He is at liberty to regard the characters by which they are represented, in the light of mere corresponding symbols, whose origin is fully explained

at their first occurrence in the table, and their ultimate application at the second. The operations included in the parentheses may be mentally effected, whenever r is a simple digit. And lastly, the vertical arrangement of the addends adapts them at once to the purposes of arithmetic, on every scale of notation.

General Synopsis.

$n-1^{th}$ Der.	$n-2^{th}$ Derivee.	$n-3^{th}$ Derivee.	...	3rd. Derivee.	2nd. Derivee.	1st. Derivee.	Synthesis.	Anal.	Root
u $r(Ur=)$	v $U(rV=)$	t V	&c.	c $D(r=)$	b $C(Cr=)$	a $B(Br=)$	$^{\phi R}$ $A(Ar=)$	$^{\Delta R}$ $-A$	$\Delta R + r + r'$
U	V	T	&c.	C	B	A	ϕR	Δ	
$n-1. r$ $(U+r^2=)$	U $(V+Ur=)$	V $(T+Vr=)$	&c.	$(D+Er=)$	D $(C+Dr=)$	C $(B+Cr=)$	B' $(A'r'=)$	A' $-A'$	
	U	V		$(D+Er=)$	D	C	B'		
	U	V		$(D+Er=)$	D	C	B'		
	&c. to	&c. to	&c.	$(D+Er=)$	D	C'		ϕR^n	Δ^n
	U	V		$(D'r'=)$	D'				
$n-2$ $r'(U'r'=)$	U' $(V'+U'r'=)$	V' $(T'+V'r'=)$		$(D'r'=)$	D'		A' $(A''r''=)$	A'' $-A''$	
	U'	V'				B' $(B'+C'r'=)$	B'	&c.	&c.
U'	V'	T'		$(D'+E'r'=)$	C' $(C'+D'r'=)$	C'	&c.		
$n-1. r'$ $(U'+r'^2=)$	U' $(V'+U'r'=)$	V' $(T'+V'r'=)$	&c.	&c.	&c.				
&c.	&c.	&c.							

Illustrations.

19. The remarks which are yet to be adduced will bear almost exclusively on the Analytic portion of the Theorem, from which the Synthetic differs only in the less intricate management of the first derivee; this function having no concern with the discovery of the root, and its multiple being additive like all the rest, instead of subtractive.

From the unrestricted nature of the notation employed, it is evident that no class of equations, whether finite, irrational or transcendental, is excluded from our design. In this respect indeed, the new method agrees with the established

popular methods of approximation ; a circumstance in favour of the latter, which is overlooked by many algebraists, both in employing those methods, and in comparing them with processes pretending to superior accuracy. The radical feature which distinguishes them from ours is this : they forego the influence of all the *derivées*, excepting the first and perhaps the second ; ours provides for the effectual action of all.

20. Concerning these *derivées* little need be said, as their nature and properties are well known. It is sufficient to state that they may be contemplated either as differential coefficients, as the limiting equations of NEWTON, or as the numerical coefficients of the transformed equation in $R + z$. This last elementary view will suffice for determining them, in most of the cases to which the popular solutions are adequate ; viz. in finite equations where R , an unambiguous limiting value of x , is readily to be conjectured. When perplexity arises in consequence of some roots being imaginary, or differing by small quantities, the second notation must be called in aid. The first, in general, when ϕx is irrational or transcendental.

21. The fact just stated, namely, that our theorem contains within itself the requisite conditions for investigating the limits, or presumptive impossibility, of the roots, demonstrates its sufficiency for effecting the developement of the real roots, independently of any previous knowledge of R . For this purpose, we might assume $R = 0$; $r, r, \&c. = 1$ or $.1 \&c.$ and adopt, as most suitable to these conditions, the algorithm of Theorem II, until we had arrived at R^* , an unambiguous limiting value of x . But since these initiatory researches

seem more naturally to depend on the simple derivees, a, b , &c. than on A, B , &c. their aggregates ; and since, in fact, as long as r is assumptive or independent of R , our system of derivation offers no peculiar advantage ; I should prefer applying the limiting formulæ in the usual way ; passing however from column to column (WOOD, § 318.) of the results, at first by means of the neat algorithm suggested in the note on Art. 7, and afterwards by differencing, &c. as recommended by LA-GRANGE, (*Res. des Eq. Num.* § 13), when the number of columns has exceeded the dimensions of the equation. (Vide Addendum.)

If, during this process the observation of DE GUA be kept in view, that whenever all the roots of ϕx are real, $D^{m-1} \phi x$ and $D^{m+1} \phi x$ will have contrary signs when $D^m \phi x$ is made to vanish, we shall seldom be under the necessity of resorting to more recondite criteria of impossibility. Every column in which o appears between results affected with like signs, will apprise us of a distinct pair of imaginary roots ; and even a horizontal change of signs, occurring between two horizontal permanences of an identical sign, will induce a suspicion, which it will in general be easy, in regard of the existing case, either to confirm or to overthrow.

22. The facilities here brought into a focus, constitute, I believe, a perfectly novel combination ; and which, on that account, as well as on account of its natural affinity to our own principles, and still more on account of the extreme degree of simplicity it confers on the practical investigation of limits, appears to merit the illustration of one or two familiar examples.

EX. 1. Has the equation $x^4 - 4x^3 + 8x^2 - 16x + 20 = 0$ any real root?—See EULER, C. D. p. 678.

x	0	1	2
	20	9	4
	-16	-8	0
	8	2	8
	-4	0	4
	1	1	1

Here the first column consists of the given co-efficients taken in reverse order. In the second, 9 is = the sum of the first column, -8 is = -16 + 2 (8) + 3 (-4) + 4 (1), 2 is = 8 + 3 (-4) + 6 (1), &c. The third column is formed from the second, by the same easy process. We need proceed no farther; for the sequences 2, 0, 1 in the second column, and 4, 0, 8 in the third, show that the equation has two pairs of imaginary roots. Consequently it has no real root.

EX. 2. To determine the nearest distinct limits of the positive roots of $x^3 - 7x + 7 = 0$. See LAGRANGE, *Res. des E. N.* § 27, and note 4. § 8.

Operating as in the former example, we have

$x =$	0	1	2
	7	1	1
	-7	-4	5
	0	3	6
	1	1	1

Since all the signs are now positive, 2 is greater than any of the positive roots. Again, between -4 and +5, it is manifest, that 0 will occur as a value of the first derivate. and

that the simultaneous value of the second derivate will be affirmative. But as the principal result has evidently converged and subsequently diverged again in this interval, no conclusion relative to the simultaneous sign of that result can be immediately drawn. We will return to complete the transformations.

For $x=$	1.0	1.1	1.2	1.3	1.4	1.5	1.6	1.7
	1000	631	328	97	-56	-125	-104	13
-	400	337	268					
	30	33	36					
	1	1	1					

Here the first column was formed from that under $x=1$, by annexing ciphers according to the dimensions of the functions; the 2nd and 3rd columns and the number 97 were found as in the former Example; the remaining numbers by differencing and extending the series 1000, 631, 328, 97. We have no need to continue the work, since the changes of signs in the principal results indicate the first digits of the roots in question to be 1.3 and 1.6. But if we proceed by farther differencing to complete all the lines, the columns standing under these numbers will give the co-efficients of $\phi(1.3 + z)$ and $\phi(1.6 + z)$ without farther trouble.

23. Assuming, then, that R has been determined, and $R + z$ substituted for x in the proposed equation, thereby transforming it to

$$\Delta = az + bz^2 + cz^3 + dz^4 + \dots$$

it is to this latter equation that the analytical part of our theorem is more immediately adapted. Now the slightest degree of reflection will evince, that our method is absolutely identical for all equations of the same order, whether they

be binomial or adfectèd, as soon as the transformation in R has been accomplished. The following description, therefore, of a familiar process in arithmetic, will convey an accurate general idea of our more extensive calculus, and obviate the necessity of any formal precepts.

In EVOLUTION, the first step is unique, and if not assisted by an effort of memory, could only be tentative. The whole subsequent process may be defined, *division by a variable divisor*. For an accurate illustration of this idea, as discoverable in the existing practice of arithmeticians, we cannot however refer to the mode of extracting any root, except that of the square; and to this, only in its most recently improved state. Here, in passing from one divisor to another, *two additive corrections* are introduced; the first depending on the last correction of the root, the second on the correction actually making. And this *new quotient correction* of the root, since it must exist previously to the completion of the divisor by which it is to be verified, *is required to be found by means of the incomplete divisor*; and may be taken out, either to one digit only, as is most usual, or to a number of digits equal to that which the complete and incomplete divisors possess in common. And farther, as these *divisors* may not, *in the first instance*, agree accurately even in a single digit, it is necessary at that stage of the operation, mentally to anticipate the effect of the new quotient, so as to obtain a sufficiently correct idea of the magnitude of the new divisor.

24. This is an accurate statement of the relation which the column headed by the first derivate bears to the analysis. The remaining columns contribute their aid, as successively subsidiary to each other; the contributions commencing with

the last or $n-1^{\text{th}}$ derivate, and being conveyed to the first through a regular system of *preparatory addends* dependent on the last quotient-correction, and of *closing addends* dependent on the new one. The *overt and registered* manner of conducting the whole calculation, enables us to derive important advantage from *anticipated corrections* of the divisors, not only at the first step, but, if requisite, through the whole performance, and also, without the necessity of a minute's by-calculation, communicates, with the result, its *verification*.

25. Let us trace the operation of the theorem as far as may be requisite, through the ascending scale of equations.

1. In *Simple equations*, the reduced equation may be represented by $\Delta = az$; whence $z = \frac{\Delta}{a}$. Now the theorem directs us to proceed thus :

$$\begin{array}{r} a \qquad \Delta (r + r' + \dots \\ \frac{-ar}{\Delta'} \\ \frac{-ar'}{\Delta''} \\ \frac{-ar''}{\Delta'''} \\ \&c. \end{array}$$

precisely the common arithmetical process of division.

2. In *Quadratics*, we have $\Delta = az + z^2$, and proceed in this manner :

$$\begin{array}{r} 1 \qquad a \qquad \Delta (r + r' + \dots \\ \frac{r}{A} \qquad \frac{-Ar}{\Delta'} \\ r' \qquad \frac{-A'r'}{\Delta''} \\ \frac{r''}{A'} \qquad \&c. \\ \&c. \end{array}$$

the known arithmetical process for extracting the square root.

3. At *Cubic equations*, the aberration of the old practice of evolution commences, and our theorem places us at once on new ground. We have here

$$\Delta = az + bz^2 + z^3$$

and must proceed thus :

1	<i>b</i>	<i>a</i>	$\Delta(r+r+\dots)$
	$\frac{r}{B}$	$Br = \frac{0}{A} B$	$\frac{-Ar}{\Delta'}$
	$2r$	$0B + r^2 = \frac{1}{A} B$	$\frac{-A'r'}{\Delta''}$
	$\frac{r'}{B'}$	$\frac{B'r'}{A'}$	$\underline{\underline{\&c.}}$
	$\&c.$	$\&c.$	

This *ought to be* the arithmetical practice of the cube root, as an example will prove.

Ex. I. *Extract the cube root of* 48228544 .

Having distributed the number into tridigital periods as usual, we immediately perceive that the first figure of the root is $3 = R$. Consequently, the first subtrahend is $R^3 = 27$, the first derivatee $3R^2 = 27$, the second $3R = 9$; the third ($=1$) need not be written. Hence

9.	27..	48228544(364
6	576	27
96	3276	21228
12.	612..	19656
4	4336	1572544
1084	393136	1572544

In this example the reader will perceive that no supplementary operations are concealed. The work before him is complete, and may be verified mentally. I need not intimate

how much more concise it is than even the abbreviated statement of the old process. (See HUTTON'S *Course*.)

The station of 1, 2, &c. numeral places respectively, which the closing addends occupy in advance of the preparatory ones, is an obvious consequence of combining the numeral relation of the successive root-figures with the potential relation of the successive derivees. In fact, as is usual in arithmetic, we tacitly regard the last root-figure as units, and the new one as a decimal fraction; then the common rules of decimal addition and multiplication regulate the vertical alinement of the addends.

26. The advantage of mental verification is common to the solution of equations of every order, provided the successive corrections of the root be simple digits: for the parenthetic derivations will, in that case, consist of multiplying a given number by a digit, and adding the successive digital products to the corresponding digits of another given number; all which may readily be done without writing a figure intermediate to these given numbers and the combined result. For this reason the procedure by single digits appears generally preferable.

Nevertheless, to assist the reader in forming his own option, and at the same time to institute a comparison with known methods on their own grounds, I introduce one example illustrative of the advantage which arises from the anticipatory correction of the divisors spoken of in Art. 24, when the object is to secure a high degree of convergency by as few approximations as possible. The example is that by which NEWTON elucidates his method. I premise as the depreciators of NEWTON do, that it is an extremely easy

Consequently,

$$1116143772)1721458218979(1542326590,22$$

This third correction is carried two places beyond the extent of the divisor, for the sake of ascertaining rigidly the degree of accuracy now attained. For this purpose, we proceed thus:

$628 \&c. \times 154 \&c. =, 968, \&c.$ is the true correction of the last divisor. Our anticipated correction was 1,000. For which if we substitute 968 &c. it will appear that our divisor should have ended, in 1,678, &c. instead of 2. The error is, 322 &c. which induces an ultimate error of (111 &c.: 154 &c.: 322 &c. &c. :), 44 &c. Consequently, our third correction should be . . . 1542326590,66, &c. agreeing to 10 figures with the value previously determined. And the root is

$$x = 2.094551481542326590, \&c.$$

correct in the 18th decimal place at three approximations.

So rapid an advance is to be expected only under very favorable data. Yet this example clearly affixes to the new method, a character of unusual boldness and certainty; advantages derived from the overt manner of conducting the work, which thus contains its own proof.

The abbreviations used in the close of this example, are of a description sufficiently obvious and inartificial; but in order to perfect the algorithm of our method in its application to higher equations, and to the progress by simple digits, attention must be given to the following general principles of

Compendious Operation.

27. We have seen that every new digit of the root occasions the resolvend to be extended n figures to the right, and the m^{th} deriver $n - m$ figures; so that if the work be carried on as with a view to unlimited progress, every new

root-figure will be obtained and verified at an expence of $\frac{1}{2}(n \cdot n + 3) + 2$ new lines of calculation, containing in all somewhat above $\frac{1}{6}(n \cdot n - 1 \cdot n + 4)$ digits more than the preceding root-figure cost. But as the necessity for unlimited continuity can rarely, if ever, occur, we may consider ourselves at liberty to check the advance of the resolvend, as soon as it contains one or two figures more than the number we yet propose to annex to the root. This will happen, generally speaking, when $\frac{1}{n}$ th of the numeral places of the root are determined.

By arresting the advance of the resolvend, we diminish it in the first instance by an optional number (p) of places, and by n places more at each succeeding step. Neglecting at the same time an equal number of figures in the right hand places of each closing addend and its derivatives, as contributing nothing to the diminished resolvend, we thus cause the effective units' place of each derivee to retrograde* in the first instance $p + m - n$ places, and at every subsequent step, a number of places (m) equal to the index of the derivee.

In the mean time, while these amputations are diminishing the derivees and addends on the right hand, a uniform average diminution of one digit on the left hand is taking place in the successive classes of addends in each column. The obvious consequence is, that after about $\frac{1}{m+1}$ th of the root-

* As the advance of the closing addend is prepared by annexing dots to the superior preparatory addend, so its retrogradation may be prepared by a perpendicular line beginning before the proper place (the m^{th} or $m + p - n^{\text{th}}$) of the said preparatory addend, and continued indefinitely downward. One digit, or two, of those which fall to the right of this line in the next succeeding sum and preparatory addends, must be ~~omitted~~, for the sake ultimately of correctly adjusting the effective units of the ~~subtrahend~~.

figures are found, the m^{th} derivee will receive no augmentation; or, in other words, it will be exterminated when $\frac{1}{m}$ th of those places are determined.

Again, when all the derivees inferior to M , the m^{th} , have vanished, the process reduces itself to that of the m^{th} order simply. For, the amount of the preparatory addends to L , the $m-1^{\text{th}}$ augmented derivee, will be $m-1$ times the previous closing addend $\circ M$; and the preparatory addends to K , the $m-2^{\text{th}}$, will be formed from its previous closing addend $\circ L$, by adding $\circ Mr$ to it $m-2$ times successively; a procedure obviously similar to that with which the general synopsis commences.

28. From these principles we form the following conclusions, demonstrative of the facilities introduced by this improvement on the original process:

1. Whatever be the dimensions (n) of the proposed equation, whose root is to be determined to a certain number of places, only $\frac{1}{n}$ th part of that number (reckoning from the point at which the highest place of the closing addend begins to advance to the right of that of the first derivee) needs to be found by means of the process peculiar to the complete order of the equation; after which, $\frac{1}{n \cdot n-1}$ may be found by the process of the $n-1^{\text{th}}$ order, $\frac{1}{n-1 \cdot n-2}$ by that of the $n-2^{\text{th}}$ order, &c.

2. Several of these inferior processes will often be passed over *per saltum*; and when this advantage ceases, or does not occur, the higher the order of the process, the fewer will be the places determinable by it. And in every case, the latter

half of the root will be found by division simply. Meantime, the number of figures employed in verification of each successive root-digit, instead of increasing, is rapidly diminishing.

3. The process with which we commence, need not be of a higher order than is indicated by the number of places to which we would extend the root; and may be even reduced to an order as much lower as we please, by means of an introductory approximation.

Ex. III. Let the root of the equation in Ex. II. be determined to the tenth place of decimals.

Arranging the derivees as before, we proceed thus:

$$\begin{array}{r}
 6.. \quad 10.... \quad 1.000000(.0945514815 \\
 \hline
 609 \quad 5481 \quad 949329 \\
 \hline
 184 \quad 105481 \quad 50671000 \\
 \hline
 62174 \quad 5562.. \quad 44517584 \\
 \hline
 .8. \quad 25096 \quad 6153416 \\
 \hline
 11129396/ \quad 5578825 \\
 25112 \\
 31412 \\
 \hline
 1115764192 \\
 31 \\
 3114 \\
 \hline
 111611 \quad 5375 \\
 3 \quad 4465 \\
 \hline
 111614 \quad 910 \\
 \hline
 \quad 898 \\
 \hline
 \quad 17 \\
 \hline
 \quad 11 \\
 \hline
 \quad 6 \\
 \hline
 \quad 6
 \end{array}$$

Consequently the root is 2.0945514815, correct to the proposed extent, as appears on comparing it with the more enlarged value already found. The work occupied a very few minutes, and may be verified by mere perusal, as not a figure was written besides those which appear. By a similar operation, in less than half an hour, I have verified the root to the whole extent found in Ex. II.

Ex. IV. As a praxis in case of the intervention of negative numbers, let it be proposed to extract, to a convenient extent, that root of the equation $x^3 - 7x = -7$, whose first digits we have already determined to be 1.3. (See Art. 22.)

Making $x = 1.3 + z$, we have

$$.097 = -1.93z + 3.9z^2 + z^3$$

Wherefore

39.	-193..	-97(.056895867
5	-1975	-86625
395	-17345	-10375000
106	2000..	-9048984
4056	24336	-1326016
12.	-1508164	-1184430
4068	24372..	-14586
	32544.	-132923
	-14805376	-8663
	32550	-7383
	366.	
	-147791	-1280
	36	-1181
	2	-99
	-147653	-89
		-10
		-10

Consequently the root is 1.356895867. This example was selected by LAGRANGE for its difficulty.

Of its three roots, that which we have now found is the most difficult to obtain; yet the whole work, including the preparatory portion in Art. 22, may be performed without one subsidiary figure, in less than a quarter of an hour.

A little attention and practice will render the mental aggregation of positive and negative numbers as familiar as the addition of either sort separately. The introduction of a small negative resolvent, instead of a large positive one, where the opportunity occurs, will then greatly abridge the operations. For example, the cube root of 2, or 1.26..

$$\begin{array}{r} -0078950105 \\ 1.259921049895 \end{array}$$

was determined to this extent, true to the 12th decimal place, within as small a compass and as short a time as the result of Example III.

Ex. V. As an example of a finite equation of a higher order, let the equation $x^4 + 2x^3 + 3x^2 + 4x + 5 = 321$ be proposed. The root appears to be > 2 , < 3 ; and the equation in $x - 2$ is

$$207 = 201x + 150x^2 + 59x^3 + 12x^4 + x^5$$

Hence,

Wherefore the root is 2.638605803327, correct to the 12th decimal, and capable, like the former results, of being verified by simple inspection. The other roots are imaginary; for when $-x = .4$, the fourth derivate vanishes between two affirmative results, and when $-x = .7$ &c., the second disappears under similar circumstances. (Arts. 21, 22.)

It appears to me, that no explanation of this solution can be offered, which has not been abundantly anticipated in this Essay; and the student who peruses it in connection with the General Synopsis, and Arts. 23, 27, will acquire an indelible impression of the whole algorithm.

Ex. VI. If it were proposed to obtain a very accurate solution of an equation of very high dimensions, or of the irrational or transcendental kind, a plan similar to the following might be adopted. Suppose, for example, the root of

$$x^x = 100, \text{ or } x \log x = 2$$

were required correct to 60 decimal places. By an easy experiment we find $x = 3.6$ nearly; and thence, by a process of the *third* order, $x = 3.597285$ more accurately.

Now, $3597286 = 98 \times 71 \times 47 \times 11$, whose logarithms, found to 61 decimals in SHARPE'S Tables, give $R \log R = 2.00000096658$, &c. correct to 7 figures; whence the subsequent functions need be taken out to 55 figures only. They are

$$a = \text{Mod} + \log R = .990269449408, \text{ \&c.}$$

$$b = \text{Mod} \div 2R = \dots\dots\dots .0760364, \text{ \&c.}$$

$$c = -b \div 3R = \dots\dots\dots -.0755, \text{ \&c.}$$

&c. The significant part disappears after the 8th derivate; consequently, the process will at first be of the *eighth* order. If the root is now made to advance by single digits, the first

of these will reduce the process to the *seventh* order; one more reduces it to the *sixth* order; two more, to the *fifth*, &c. The last 27 figures will be found by division alone.

But if the first additional correction is taken to 8 figures, and the second to 16, on the principle of Example II, we pass from the 8th order to the 4th at once, and thence to the 1st or mere division, which will give the remaining 29 figures. This mode appears in description to possess the greater simplicity, but is perhaps the more laborious.

It cannot fail to be observed, that in all these examples a great proportion of the whole labour of solution is expended on the comparatively small portion of the root, which is connected with the leading process. The toil attending this part of the solution, in examples similar in kind to the last, is very considerable; since every deriuee is at this stage to receive its utmost digital extent. To obviate an unjust prejudice, I must therefore invite the reader's candid attention to the following particulars:

In all other methods the difficulty increases with the extent of the root, nearly through the whole work; in ours, it is in a great measure surmounted at the first step: in most others, there is a periodical recurrence to first conditions, under circumstances of accumulating inconvenience; in the new method, the given conditions affect the first deriuees alone, and the remaining process is *arithmetically direct*, and increasingly easy to the end.

The question of practical facility may be decided by a very simple criterion; by comparing the *times* of calculation which I have specified, with a similar datum by Dr. HALLEY in favor of his own favorite method of approximation. (Philosophical Transactions for 1694.)

Addendum I. (*Vide* Art. 21.) *Note.* But in this case, it will be more elegant to find the differences at once by the theorem $\Delta^{t+1}D^m\phi R' = \frac{m+1}{1}\Delta^tD^{m+1}\phi R.r + \frac{m+1}{1} \cdot \frac{m+2}{2}\Delta^tD^{m+2}\phi R.r^2 + \&c.$ which, supposing r to be constant, is a sufficiently obvious corollary to the theorem in Art. 7. All the results may then be derived from the first column by addition. Thus, for the latter transformations in Ex. II. Art. 22, the preparatory operation would be

1st. Terms.	Diff. 1st.	2nd.	3rd.
1000	—369	66	6
—400	63	6	
30	3		

and the succeeding terms would be found by adding these differences in the usual way to the respective first terms.

Addendum II. It is with pleasure that I refer to the Imperial Encyclopædia (Art. Arithmetic) for an improved method of extracting the cube root, which should have been noticed in the proper place, had I been aware of its existence; but it was pointed out to me, for the first time, by the discoverer, Mr. EXLEY, of Bristol, after this Essay was completed. It agrees in substance with the method deduced in Art. 25, from my general principle, and affords an additional illustration of the affinity between that principle and the most improved processes of common arithmetic.

PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

FOR THE YEAR MDCCCXIX.

PART III.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, Pall-mall, BOOKSELLERS TO HIS MAJESTY,
AND PRINTERS TO THE ROYAL SOCIETY.

MDCCCXIX.

CONTENTS.

XXII. *An account of experiments for determining the variation in the length of the pendulum vibrating seconds, at the principal stations of the Trigonometrical Survey of Great Britain.*

By Capt. HENRY KATER, F. R. S. - - - - - p. 337

List of Presents, - - - - - p. 509

Index.

PHILOSOPHICAL TRANSACTIONS.

XXII. *An account of experiments for determining the variation in the length of the pendulum vibrating seconds, at the principal stations of the Trigonometrical Survey of Great Britain.*
By Capt. HENRY KATER, F. R. S.

Read June 24, 1819.

BEFORE I enter upon a detail of the operations which form the subject of this Paper, it may not perhaps be improper to give a brief statement of the occasion to which they owe their origin.

The subject of weights and measures having for some time past been before the British Parliament, an Address was presented to His Royal Highness the Prince Regent, in pursuance of a resolution of the House of Commons of the 15th of March 1816, to the following effect.

“ Resolved, that an humble address be presented to His
“ Royal Highness the Prince Regent, that he will be gra-
“ ciously pleased to give directions for ascertaining the length
“ of the pendulum vibrating seconds of time in the latitude
“ of London, as compared with the standard measure in the

“ possession of this house, and for determining the variations
 “ in length of the said pendulum, at the principal stations of
 “ the Trigonometrical Survey extended through Great Bri-
 “ tain; and also for comparing the said standard measures,
 “ with the ten millionth part of the quadrant of the meridian,
 “ now used as the basis of linear measure on (*a part of*) the
 “ continent of Europe.”

In consequence of His Royal Highness's compliance with the prayer of this Address, an application was made by His Majesty's Ministers to the Right Honourable Sir JOSEPH BANKS, requesting that the Royal Society would be pleased to afford all the assistance in their power for the accomplishment of the objects therein mentioned; and a Committee was appointed for that purpose, of which I was named a member.

The length of the pendulum vibrating seconds in the latitude of London, and that of the French *mètre* having been determined, it remained to ascertain the length of the pendulum at the principal stations of the Trigonometrical Survey.

This work the Royal Society did me the honour to request I would undertake; and the ready compliance of Government with every requisition I made through Sir JOSEPH BANKS, for that assistance without which my success might have been doubtful, led me to devote with pleasure my time and labour to this highly interesting enquiry.

The instruments with which I provided myself were, a transit by DOLLAND, of three feet and a half in length, constructed on the same principle as the transit at the Royal Observatory at Greenwich, so as admirably to combine lightness with strength.

A repeating circle of one foot diameter by TROUGHTON,

A clock and a box chronometer by ARNOLD, for the loan of which I was indebted to HENRY BROWNE, Esq. F. R. S. and

An invariable pendulum with its support, a description of which will be given hereafter. To these was added, a chest of tools of various kinds.

A small light waggon was constructed at the Royal Arsenal at Woolwich, for the conveyance of these instruments, and a party consisting of a non-commissioned officer, two gunners, (one a carpenter), and two drivers with four horses of the Royal Artillery, was placed under my orders: a bell tent, and two others of a smaller description, were issued, which I found particularly useful.

His Royal Highness the Commander in Chief was pleased to direct that I should receive such military assistance as might be necessary for the safety of the instruments at the different stations, and for the use of barracks, where I might find them suited to my experiments; and an application being also made to the Admiralty for a vessel to convey me to the Shetland Islands, His Majesty's sloop of war the Cherokee, commanded by Capt. T. SMITH, was ordered to receive me at Leith, and to bring me back to Scotland.

Thus liberally provided with all that could tend to facilitate the success of my undertaking, I left London on the 24th June with Lieut. FRANKS of the Royal Navy, a gentleman whose fondness for science induced him to accompany me, and arrived at Leith on the evening of the 28th.

Here on enquiry I found that the Cherokee had not been heard of for some time, but the Admiralty having ordered that any requisition I made should be complied with,

and His Majesty's sloop the *Nimrod*, commanded by Capt. DALLING, being in the harbour, she was directed to prepare immediately for sea, and on the 1st July, her provisions being completed, I embarked for Unst.

Having put into Lerwick for two days, I availed myself of the opportunity to present a letter of introduction to Dr. EDMONDSTONE, and to obtain one from him to his brother THOMAS EDMONDSTONE, Esq. of Unst, to whose hospitality I was aware I must be indebted during my stay on that Island.

On the 9th July we arrived at Unst, having been joined on the voyage by the *Cherokee*, bearing an order from the Admiral commanding at Leith to relieve the *Nimrod*. To both Capt. DALLING and Capt. SMITH I feel myself much indebted, not only for their judicious arrangements for the safety of the instruments, but also for the personal kindness and attention I experienced from them.

At Unst, I was welcomed on the beach by Mr. EDMONDSTONE, who had received notice from his brother of my intended visit; and I immediately proceeded to examine the buildings which surrounded this gentleman's house, to select a place proper for my experiments. I at length chose the shell of an unfinished cottage nearly adjoining to the cow-house, in which the preceding summer M. BIOT had made his observations on the pendulum when he visited Shetland on the part of the Institute of France. One wall of this cottage, upwards of three feet thick, was ancient, though the rest of the building was modern, and it seemed to promise sufficient stability for my purpose.

It is now necessary to give a description of the apparatus I employed.

The pendulum was composed of a bar of plate brass 1,6 inches wide, and rather less than the eighth of an inch thick. These dimensions were chosen that the pendulum and the thermometer placed near it, might be affected with equal readiness by any change of temperature. A flat circular weight nicely turned, and pierced in the direction of its diameter to receive the bar, was slid upon it, and fastened with screws and rivets at such a distance from the knife edge which served as the point of suspension, and which will presently be described, as that the pendulum made two vibrations less than the pendulum of the clock, in eight or nine minutes. The inside of the weight having been previously tinned, it was exposed to a sufficient degree of heat to solder it to the bar.

That part of the bar which was below the weight, was reduced to the width of 0,7 inch, and covered with black varnish, in order to enable me the better to observe its coincidence with the pendulum of the clock, in the manner which has been fully described in the Philosophical Transactions for 1818, in an "Account of Experiments on the length of the Pendulum vibrating seconds in the latitude of London." With the contents of this Paper I shall suppose a previous acquaintance, as an occasional reference to it will save much repetition.

To the top of the bar, a strong cross piece of brass was firmly rivetted and soldered, and a triangular hole having been made in the bar, a knife edge was passed through it, and a perfect contact between the back of the knife edge and the cross piece was insured by grinding them together. It was then secured in its place by two screws, the heads of

which were sunk in the cross piece, and having been warmed, were dipped in pitch to prevent the possibility of their being loosened by the motion of the waggon.

The knife edge was made of wootz, precisely in the same manner as described in the experiments on the length of the seconds pendulum, its ultimate angle being about 120° . The length of the bar from the knife edge to the extremity was about five feet, and it terminated in an obtuse angle, serving to indicate the arc of vibration. The weight of the whole pendulum was 15 lb. 2 oz.

The perfect immobility of the point of suspension being of the utmost consequence, every precaution was taken by the arrangement of the form, and by the weight of the frame destined to carry the pendulum, to oppose the lateral force which might result from its vibrations.

The frame was of cast iron; the horizontal part was 19 inches long, 17 wide, and half an inch thick. The back, three inches in width, at right angles to the length was pierced with three equi-distant holes in the horizontal direction, to receive very large screws about five inches long, with coarse threads destined to attach the frame to pickets of wood driven into a wall. Two brackets were firmly screwed to the under part of the horizontal frame; these brackets were bevelled so as to spread at the bottom to the width of three feet, thereby opposing more effectually any disposition to lateral motion. In the lower extremities of the brackets, two holes were made for screws similar to those above mentioned. The weight of the frame was 87 lb.

A bell metal support, furnished with agate planes on which the knife edge of the pendulum was to rest, varied but little

from that described in the Philosophical Transactions before referred to. It was contrived in such a manner as to be attached to the iron frame by three screws, and was levelled by placing thin sheets of lead between it and the frame, a method which was preferred from its promising a great degree of firmness.

An arc divided into degrees and tenths for ascertaining the extent of the vibrations of the pendulum, was attached to a piece of wood which fitted into the opening of the door of the clock case.

Expansion of the pendulum.

When the bar of the pendulum was prepared, previous to the weight being soldered to it, its expansion was determined in the same manner as is described in the Philosophical Transactions before referred to. The results were as follow :

Distance between the lines on the Bar 39,54 inches.				
Highest Temp.	Lowest Temp.	Diff. of Temp.	Div. of Microm.	Expansion in parts of the length for each degree.
°	°	°		
125,0	56,3	68,7	648	,00001022
125,0	99,0	26,0	245	,00001021
99,0	73,8	25,2	220	,00000946
73,8	63,0	10,5	91	,00000938
Mean				,00000982

.Hence the expansion of the pendulum appears to be ,0000982 parts of its length for each degree of the thermometer; and the corresponding correction to be applied to the number of vibrations in 24 hours for such change of temperature will be 0,423.

Operations at Unst.

I have remarked, that I selected for my experiments at Unst, an unfinished cottage, one of the walls of which was three feet thick. This was composed of irregular masses of serpentine, which I feared might be loosened by driving in the pickets to which the iron frame was to be screwed. Happily, however, I found the pickets act as wedges, and secure the stones more firmly in their places. The pickets driven into the wall were of oak, and were upwards of three inches in diameter, and more than a foot in length. To these the iron frame was firmly attached by its five screws, and on the evening of the 10th of July, I had the satisfaction of finding it as securely fixed as I could possibly desire.

Two pieces of deal plank two inches and a half thick, were next fastened by long nails to the wall. To these the clock case was screwed at such a distance beneath the iron frame, as that the end of the brass pendulum might reach a little below the centre of the pendulum of the clock, and the clock was then put *in beat*, by moving the bottom of the case to the right or left, and when properly adjusted, the screws were tightened. The bell metal support was next put in its place and carefully levelled, and the pendulum lodged in the Ys elevated for that purpose.

The triangular stand carrying the telescope, described in the paper on the seconds pendulum before referred to, was firmly screwed to pickets driven into the ground at about eight feet and a half in front of the clock; and the Ys which supported the pendulum being lowered till the knife edge rested on the agate planes, the diaphragm of the telescope was

adjusted so as for its edges to coincide exactly with those of the extremity of the pendulum. The next step was to bring in a right line, the telescope, the extremity of the pendulum, and a white circle of the same diameter pasted on a black ground on the centre of the pendulum of the clock. For this purpose both pendulums being at rest, the telescope was slid laterally on its support* until a small particle of the disk was seen, and a mark was made on the support of the telescope with a pencil. The telescope was now slid in the opposite direction till an equal portion of the disk became visible, when another mark was made, and the telescope being placed so as to bisect these two marks, the centre of the object glass would evidently be in the prolongation of a line joining the white disk and the extremity of the pendulum.

The diaphragm was next brought by the circular horizontal movement of the telescope to correspond with the edges of the pendulum, and the divided arc for indicating the extent of the vibrations was placed so that its zero coincided with the extremity of the pendulum.

The same thermometer which was used in my former experiments and for the loan of which I was indebted to the kindness of Dr. WOLLASTON, was suspended on the clock case near the middle of the pendulum, and every thing being thus arranged, the pendulum of the clock was put in motion, and the knife edge elevated by means of the Ys above the agate planes, to prevent any injury when not in use.

A firm support for the transit instrument became the next object of attention, and for this purpose I tried a box nearly

* The wooden support was placed so as for the telescope to be within the limits of the sliding adjustment.

filled with sand, upon which a flat stone was laid. But as this did not prove so steady as I expected, a larger stone was afterwards procured and laid upon the box, and upon this the transit was placed.

The bell tent before mentioned was suspended over the transit from three spars lashed together at the top.

The *interval* of time between the transits of the same star being all that is required for the present purpose, it is not necessary that the transit instrument should be accurately in the meridian; it is sufficient that it should always describe the same vertical circle; it was however brought very near the meridian, at all the stations, by the following method:

The error of the chronometer was determined by altitudes of the sun, and the times were computed when the first and last limb would be on the meridian.

The axis of the transit was carefully levelled, and a little before the time of the sun's first limb coming to the meridian, the middle wire of the transit was brought in contact with it, and kept so by the horizontal adjustment till the calculated time of its arrival on the meridian. The position of the instrument was afterwards farther corrected if necessary by the transit of the second limb. At other of the stations, when the weather permitted, the instrument was brought extremely near the meridian by the transit of the pole star, the telescope being sufficiently powerful to command this star with ease, at any time of the day.

A mark (generally a flat board sharpened at one end to penetrate the ground) was sent to as great a distance as convenient, and so placed by signal, that it was bisected by the middle wire of the transit; and to this the instrument was

carefully adjusted previously to every observation. The preceding detail may serve, with very little difference, for each of the stations, and I have been thus minute in my description of the various adjustments necessary, in order that no difficulty may be experienced by any who may use the pendulum after me.

In observing the time of the transits, the chronometer was used, and was found to be particularly convenient from its beating half seconds. As soon as possible after the passage of the star, the chronometer was carefully compared with the clock, and the difference being applied to the time of the transit shown by the chronometer, and also the computed gain or loss of the clock during the interval between the observation and the comparison; the time shown by the clock at the instant of the transit was obtained.

These comparisons, as well as the whole of the data necessary for the examination of the results given in this paper, will be found in the Appendix.

The climate of Unst, at the season when I visited it, is such as to render the opportunities for celestial observations extremely rare. I had been informed, that the months of July and August were the most favourable, but on the contrary, I learnt on my arrival that they were considered the least so of any of the year, the atmosphere being generally clearest in May and September. Dense fogs and light rains succeeded each other, rarely permitting a sight of the sun; and it was not until the 22d of July, that I was able first to observe the transits of a few stars.

The following table contains the observations for the rate of the clock at Unst, derived from the table of transits given in the Appendix.

Transits observed at UNST.						
Star.	July 22.	July 24.	July 25.	July 26.	July 27.	July 28.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
*The Sun		0.13.59.32		0.15.41.63		0.17.25.11
Arcturus	6.14.18.14					8.55.18.02
α Ophiuchi			9.23.43.56			9.14.33.51
ν Ophiuchi	9.55.36.41		9.46.18.11			9.33.34.06
γ Serpentis	10.18.25.08		10. 9. 6.1			9.59.22.73
α Lyrae	10.37.5		10.27.41.82			10.18.32.11
α Orionis	21.50.15.3				21.34.18.60	

From the above data the following rates of the clock were obtained, by dividing the difference between the times of the transits of each star by the interval in days, and subtracting this from $3^m.55^s.91$, the acceleration of the fixed stars in 24 hours. To this, which is the rate of the clock in a sidereal day, the gain of the clock ($0^s.14$) in four minutes was added, to obtain the rate for a mean solar day.

Rate of the clock at UNST. (Gaining.)							
Star.	From 22 to 23.	From 23 to 24.	From 24 to 25.	From 25 to 26.	From 26 to 27.	From 27 to 28.	From 28 to 29.
The Sun					51.10	50.10	52.09
Arcturus	50.57						
α Ophiuchi				51.70			
ν Ophiuchi	50.73		49.95	51.41			
γ Serpentis	50.66		49.72	51.59			
α Lyrae	50.57		49.32	51.81			
α Orionis		50.73					
Mean by the Stars	50.63	50.73	49.66	51.63			
Mean by the Sun.					51.10	50.10	52.09

* To the observations of the sun the equation of time must always be applied, in order to obtain the rate of the clock.

On the 23d July I began to observe coincidences, in the manner described in my Paper on the length of the seconds pendulum. Two series, each of ten intervals were taken each day; these are given at large in the Appendix, the results were as follow :

Vibrations of the pendulum at UNST. The clock making 86450.63 vibrations in a mean solar day.						
Date.		Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours, at 62 degs.
July 23	P. M.	30.00	58.4	86093.78	1.52	86092.26
	P. M.	30.30	59.3	86093.14	1.14	86092.00
24	A. M.	29.90	57.3	86093.33	1.99	86091.34
	P. M.	29.82	59.7	86092.45	0.97	86091.48
25	A. M.	29.84	57.7	86093.12	1.82	86091.30
	P. M.	29.72	59.0	86092.24	1.27	86090.97
26	A. M.	29.95	57.8	86092.37	1.78	86090.59
	The scapement was oiled without stopping the clock.					
27	A. M.	29.95	56.8	86091.69	2.20	86089.49
	P. M.	30.00	57.2	86091.62	2.03	86089.59
28	A. M.	30.15	54.3	86092.51	3.26	86089.25
	P. M.	30.20	58.0	86091.57	1.69	86089.88
Mean		29.98	57.8			86090.74

The numbers in the above Table are deduced from the rate of the clock (gaining 50.63) between the 22d and 28th of July. For any other interval and rate, the mean of the vibrations during such interval is taken, and the difference between the corresponding rate and 50.63 is added to, or subtracted from such mean number of vibrations accordingly as the rate of the clock has increased or diminished. The same method is pursued in all the subsequent experiments. In this manner the results contained in the next following table under the head of "computed vibrations in a mean solar day" were obtained.

The invariable pendulum furnishes a means of severely checking the rate of the clock; for should any alteration occur, it immediately indicates it. Thus on referring to the preceding table of "vibrations of the pendulum at 62° ," it appears that from the 23d to the 28th of July, a gradual increase in the rate of the clock had taken place, amounting in the whole to a quantity equal to 2,5 vibrations of the pendulum, or 0,5 of a vibration in every 24 hours.

The rate of the clock, is that due to the *middle time* of the interval between the transits from which it is deduced. The number of vibrations of the pendulum is obtained for the *mean* of the times at which the coincidences were observed. If this mean should not coincide with the time for which the rate of the clock is obtained, and the rate of the clock should be variable, the number of vibrations of the pendulum computed on such given rate must evidently be erroneous. If the mean of the interval of the transits should be *before* the mean of the times of the coincidences, the number of vibrations will, in the present case of an accelerating rate, be in *defect*. If *after* the mean of the coincidences, they will be in *excess*; and the proportionate change of rate must be added or subtracted accordingly. On this principle the corrections were calculated and the results obtained, which are contained in the following table.

By the Stars. UNST.							
From	To	Computed Vibrations in a mean solar day.	Mean of Transits B or A coincidences.	Correc- tion.	Corrected Vibrations in a mean solar day.	No. of Stars observed.	Inter- of Transit.
July. 23 P. M.	July. 28 P. M.	86090,71	h. m. B. 1. 27	+ ,03	86090,74	4	6
23 P. M.	28 A. M.	86090,93	A. 1. 58	— ,04	86090,89	1	5
23 P. M.	25 P. M.	86090,59	B. 2. 37	+ ,05	86090,64	3	3
26 A. M.	28 P. M.	86090,76	B. 5. 52	+ ,12	86090,88	4	3
By the Sun.							
24 P. M.	28 A. M.	86090,85	A. 1. 20	— ,03	86090,82	2	4
24 P. M.	26 A. M.	86090,55	A. 0. 58	— ,02	86090,53	2	2
26 P. M.	28 A. M.	86090,90	B. 6. 11	+ ,12	86091,02	2	2

We have now to consider what authority attaches to each result, so that we may employ all the observations in obtaining a mean, and yet give to each set that degree of weight only to which it is entitled.

The accuracy of any one result will evidently in the first place depend on the number of stars observed from which the rate of the clock is deduced ; and on this head as may be seen by examining the table of transits, there is little probability of serious error.

But the position of the transit instrument with respect to the meridian mark, requires the most minute care, and I soon discovered that to this, and to the accurate levelling of the axis, it was necessary to pay unceasing attention, as a deviation equal to the diameter of the silkworm's thread in the focus of the eye glass, would occasion an error in the time of the transit of a star amounting to about three tenths of a second.

The effect of this error on the daily rate of the clock, is lessened in proportion to the number of days comprised between the two transits; for if the rate of the clock be deduced from transits observed on two successive days, the whole amount of the error arising from any deviation of the instrument from the meridian mark, will be included in the rate; but for any longer interval, it is divided by the number of days constituting such interval.

In order therefore to obtain a true mean, it appears that each result should be multiplied by the product of the number of the stars into the interval between the observations, and the sum of such final products be divided by the sum of the factors.

Observations of the sun are perhaps less entitled to credit than those of the stars, as in consequence of an apparent wavering of the meridian mark, some degree of uncertainty frequently exists in adjusting the transit instrument; setting this aside, a transit of both limbs of the sun may be considered equal to the transits of two stars.

Proceeding in the computation in the manner just described, we obtain 86090,77 vibrations of the pendulum in 24 hours, by the observations of the stars, and 86090,79 by those of the sun. But from what has been said, these results are entitled to credit in the ratio of the sums of their factors, that is, as 50 to 16; the final mean is therefore 86090,77 vibrations in a mean solar day.

The force of gravity decreasing as the square of the distance from the earth's centre increases, the next step is to find the correction on this supposition for the height of the station above the level of the sea. As the square of the

number of vibrations of the pendulum represents the force of gravity, we have this simple rule: convert the height of the station into the decimal of a mile, and divide it by the radius of the earth (3954.583) the quotient is the factor by which the number of vibrations in 24 hours being multiplied, the product will be the correction required.

But the quantity thus obtained is evidently erroneous, being founded on the supposition that the experiments are made on an elevation having no attractive matter surrounding it; and it is observed by Dr. YOUNG, in a letter which that eminent mathematician addressed to me, and which is published in the Phil. Trans. for 1819, entitled "Remarks on the probabilities of error in physical observations, and on the density of the earth, considered especially with regard to the reduction of experiments on the pendulum;" that "if we were raised on a sphere of earth a mile in diameter, its attraction would be about $\frac{1}{8000}$ of that of the whole globe, and instead of a reduction of $\frac{1}{8000}$ in the force of gravity, we should obtain only $\frac{3}{8000}$, or $\frac{3}{4}$ as much. Nor is it at all probable, that the attraction of any hill, a mile in height, would be so little as this, even supposing its density to be only two thirds of the mean density of the earth. That of a hemispherical hill of the same height would be more than half as much more (*than the sphere*) or in the proportion of 1,586 to 1. And it may be easily shown, that the attraction of a large tract of table land, considered as an extensive flat surface a mile in thickness, would be three times as great as that of a sphere a mile in diameter; or about twice as great as that of such a sphere of the mean density of the earth: so that, for a place so situated, the allowance for elevation would be reduced to one half: and in almost any country that could

“ be chosen for the experiment, it must remain less than three
 “ fourths of the whole correction deduced immediately from
 “ the duplicate proportion of the distances from the earth's
 “ centre.”

By this interesting, and I believe new view which Dr. YOUNG has taken of the subject, it appears that the correction for the elevation above the sea, will vary (according to the nature of the eminence and also its density) from one half to three fourths of the quantity before deduced from the squares of the distances from the earth's centre, and if the mean density of the earth be taken at 5,5, and that of the matter surrounding the station at 2,5, Dr. YOUNG is of opinion, that the quantity deduced from the duplicate ratio of the distances should be multiplied by $\frac{66}{100}$, to obtain the correction for a table land, and by $\frac{7}{10}$ for that of an eminence of moderate declivity.

By careful levelling, the height of the station at Unst above low water, was found to be 28 feet; whence we have 0,12 for the correction deduced from the squares of the distances from the earth's centre, and as the station at Unst was surrounded by hills composed of serpentine, I shall take $0,12 \times 1 = 0,06$ for the correction to be applied in order to obtain the number of vibrations which would be made at the level of the sea.

The last correction to be found, is for the buoyancy of the atmosphere. The manner in which this correction is derived, has been fully explained in the “ Account of experiments for determining the length of the seconds pendulum ” before referred to. The specific gravities of the weight and bar of the pendulum, were carefully determined. That of the bar was found to be 8,628, and of the weight 8,603. The specific

gravity therefore of the whole pendulum may be taken at 8,610.

The mean height of the barometer during the experiments at Unst, was 29,98 inches, and that of the thermometer $57^{\circ}8$. The weight of water is to that of air at 29,27 inches of the barometer, and 53° of the thermometer, as 836 to 1, and the expansion of air for each degree of the thermometer is $\frac{1}{480}$ of its bulk. From these data we find that the specific gravity of the pendulum was to that of air, at the time of the experiments, as 7099 to 1. The square of the number of vibrations must therefore be increased $\frac{1}{7099}$ part, or 6,07 be added to the number of vibrations in 24 hours, to obtain the number of vibrations which would be made during the same period in vacuo.

These corrections being added to the mean number of vibrations before given, we have 86096,90 for the number of vibrations made by the pendulum in a mean solar day, in vacuo at the level of the sea.

The very unfavourable weather which I experienced at Unst, prevented my obtaining so many observations for the rate of the clock, as I could have wished; but though the greatest difference between the seven resulting numbers of vibrations amounts to so much as 0,49, I think it probable, after a careful examination, that the final result must be within one tenth of a vibration of the truth.

On the 23d July, I was so fortunate as to obtain one series of meridional observations of the sun, with the repeating circle, for the latitude of the station, which will be given hereafter, and on the 29th I embarked on board the Cherokee, and took leave of my kind host Mr. EDMONDSTONE, to whose most friendly hospitality the eloquent pen of M. Bior has

done but justice, and has left me nothing to add, but that I experienced from him every attention that could contribute to my personal comfort, and every anxious exertion that could tend to forward the enquiry in which I was engaged.

Operations at Portsoy.

On the first of August I arrived at Portsoy, near to which is Cowhythe, the next station of the trigonometrical survey, which I proposed to connect with my observations, and after much search for a place suited to my experiments, was kindly favoured by the Rev. Mr. GRANT, with the use of his school-house, which was perfectly adapted to the purpose, the walls being thick, and firmly built of serpentine. I was also so fortunate as to obtain accommodations for myself, at a house belonging to a gentleman of the name of WATSON, immediately adjoining the school-house, and whose garden afforded an excellent situation for the transit instrument.

On the 5th August I commenced the observations detailed in the Appendix, from which is extracted the following table for obtaining the rate of the clock :

Transits observed at PORTSOY. 1st Series.							
Stars.	August 5.	August 6.	August 7.	August 8.	August 10.	August 11.	August 12.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
The Sun	. . .	0.14.41.62	0.15. 7.95	0.15.36.56	0.16.38.83	. . .	0.17.42.63
Arcturus	5.11.20.51	5. 4.46.51	. . .	4.58.16.63
α Ophiuchi	. . .	8.36.42.60	. . .	8.30. 1.45	8.23.28.67	8.20.13.59	8.16.58.52
γ Ophiuchi	9. 2.35.99	8.52.29.70	8.45.56.85	8.42.41.94	8.39.26.94
γ Serpentis	9.25.25.99	9.15.19.73	9. 8.46.76	9. 5.32.01	9. 2.17.04
α Lyrae	9.44.16.20	9.34.10.19	9.27.37.34	9.24.22.52	9.21. 7.43
a	9.53.22.62	9.50. 7.82	9.46.52.91
b	10.16.29.86	9.59.51.24	9.56.36.52	9.53.21.61
μ Aquilæ	10.38.33.58	10.18.40.52	10.15.25.11
α Aquilæ	10.55.13.62	10.45. 8.14	10.38.35.49	10.35.20.47	10.32. 5.33

From the above data the following rates of the clock were
before fully particu-
larized.

Rate of the clock at PORTSMOUTH, 1st Series.—(Gaining.)

Stars.	From 5 to 8.	From 5 to 10.	From 5 to 11.	From 5 to 12.	From 6 to 7.	From 6 to 8.	From 6 to 10.	From 6 to 12.	From 7 to 8.	From 7 to 10.	From 7 to 12.	From 8 to 10.	From 8 to 11.	From 8 to 12.	From 10 to 11.	From 10 to 12.	From 11 to 12.
The Sun	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
Arcturus	—	—	—	—	32.73	34.22	36.63	38.10	35.71	37.93	39.18	39.03	—	40.04	—	41.05	—
Opitchi	—	—	—	—	—	—	—	—	—	—	—	39.02	—	40.05	—	41.09	—
Opitchi	—	—	—	—	—	35.44	—	—	—	—	—	39.63	40.07	40.29	40.95	40.96	40.96
Serpentis	33.90	36.18	37.00	37.57	—	—	—	—	—	—	—	39.59	40.10	40.33	41.12	41.08	41.03
Lyra	33.91	36.16	37.02	37.59	—	—	—	—	—	—	—	39.54	40.12	40.35	41.30	41.17	41.04
a	34.00	36.24	37.06	37.61	—	—	—	—	—	—	—	39.59	40.13	40.33	41.21	41.08	40.94
b	—	—	—	—	—	—	—	—	—	—	—	—	—	—	41.23	41.18	41.12
Aquilæ	—	36.29	37.12	37.69	—	—	—	—	—	—	—	—	—	—	41.31	41.22	40.62
Aquilæ	34.17	36.38	37.15	37.68	—	—	—	—	—	—	—	39.69	40.13	40.15	41.01	40.95	40.89
Mean by the Stars	34.00	36.35	37.09	37.63	—	35.44	—	—	—	—	—	39.61	40.11	40.29	41.16	41.09	40.96
Mean by the Sun	—	—	—	—	32.73	34.22	36.63	38.10	35.71	37.93	39.18	39.03	—	40.04	—	41.05	—

* These are rejected.

From the detail of the coincidences observed at Portsoy given in the Appendix, and from the rate of the clock from the 5th to the 12th, is derived the following Table.

Vibrations of the Pendulum at PORTSOY. 1st. Series. The clock making 86437,63 vibrations in a mean solar day.						
Date.		Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Vibration in 1 hour, at 62 degrees.
Aug. 6	A. M.	29,95	64,8	86085,53	1,18	86086,71
	P. M.	30,00	65,2	86084,19	1,35	86085,54
7	A. M.	29,89	62,3	86083,09	0,13	86083,22
	P. M.	29,88	62,6	86082,30	0,25	86082,55
8	A. M.	30,05	58,8	86081,61	1,35	86080,26
	P. M.	30,09	60,5	86081,11	0,63	86080,48
9	A. M.	30,04	60,4	86080,13	0,68	86079,45
	P. M.	30,04	60,5	86078,63	0,63	86078,00
10	A. M.	30,10	58,8	86078,39	1,35	86077,04
	P. M.	30,16	60,3	86077,56	0,72	86076,84
11	A. M.	30,28	56,6	86078,44	2,28	86076,16
	P. M.	30,27	60,0	86077,34	0,85	86076,49
12	A. M.	30,26	59,2	86076,92	1,18	86075,74
	P. M.	30,27	61,3	86076,51	0,30	86076,21
Mean	.	30,09	60,8			86079,62

On examining the preceding Table, it appears that the rate of the clock had pretty regularly increased to the surprising amount of 10¹,51 in the space of 7 days; which is an acceleration of 1¹,5 in every 24 hours; on this I shall have occasion to remark hereafter. From the foregoing data the following Table of the corrected vibrations of the pendulum in a mean solar day was computed, in the manner which has been before detailed.

By the Stars, PORTSOY—1ST Series.

From	To	Computed Vibrations in a mean solar day.	Mean of Transits B or A coincidences.	Correc-tion.	Corrected Vibrations in a mean solar day.	No. of Stars observed.	Inter. of Transits.
August.	August.		h. m				
6 A. M.	8 P. M.	86079,50	B. 1.17	+0,08	86079,58	4	3
6 A. M.	10 P. M.	86079,63	B. 1.17	+0,08	86079,71	5	5
6 A. M.	11 P. M.	86079,69	B. 1.14	+0,08	86079,77	6	6
6 A. M.	12 P. M.	86079,62	B. 1.13	+0,08	86079,70	6	7
7 A. M.	8 P. M.	86079,44	B. 2.23	+0,16	86079,60	1	2
9 A. M.	10 P. M.	86079,81	B. 1.42	+0,10	86079,91	6	2
9 A. M.	11 P. M.	86079,81	B. 1.49	+0,11	86079,92	5	3
9 A. M.	12 P. M.	86079,65	B. 1.50	+0,11	86079,76	5	4
11 A. M.	11 P. M.	86079,86	B. 1.50	+0,11	86079,97	7	1
11 A. M.	12 P. M.	86079,61	B. 2.18	+0,14	86079,75	8	2
12 A. M.	12 P. M.	86079,30	B. 1.31	+0,09	86079,39	8	1

By the Sun. 1st Series.

6 P. M.	7 A. M.	86079,48	A. 1.22	—0,08	86079,40	2	1
6 P. M.	8 A. M.	86079,48	A. 1.20	—0,08	86079,40	2	2
6 P. M.	10 A. M.	86079,82	A. 1.21	—0,08	86079,74	2	4
6 P. M.	12 A. M.	86079,78	A. 1.15	—0,08	86079,70	2	6
7 P. M.	8 A. M.	86079,48	A. 1.19	—0,08	86079,40	2	1
7 P. M.	10 A. M.	86079,93	A. 1.19	—0,08	86079,85	2	3
7 P. M.	12 A. M.	86079,85	A. 1.14	—0,08	86079,77	2	5
8 P. M.	10 A. M.	86080,14	A. 1.20	—0,08	86080,06	2	2
8 P. M.	12 A. M.	86079,93	A. 1.13	—0,08	86079,85	2	4
10 P. M.	12 A. M.	86079,73	A. 1. 4	—0,07	86079,66	2	2

By using the number of stars observed and the intervals between the transits, to obtain a mean, in the manner described in the account of the experiments at Unst, we have 86079,74 vibrations by the observations of the stars, and 86079,73 by those of the sun; whence is derived 86079,74 for the final mean number of vibrations in 24 hours.

The height of the pendulum at Portsoy, above low water, was found by levelling to be 94 feet, the correction due to which is $0,39 \times \frac{1}{10} = 0,29$.

* It may be necessary to remark, that no allowance has been attempted for any variation of density between the different stations, but solely for their form.

The mean height of the barometer during the experiments, was 30.09 inches, and the mean temperature $60^{\circ}.8$, from which data, and the specific gravity of the pendulum, we have 6.04 for the correction, on account of the buoyancy of the atmosphere.

Applying these corrections to the mean number of vibrations before found, we obtain 86086.01 for the final number of vibrations which would be made by the pendulum in a mean solar day, in vacuo, and at the level of the sea.

The rate of the clock having suffered a continual acceleration, as I have before stated, it became a subject of anxious importance to determine what effect this might possibly have on the result of the experiments; particularly as the same curious circumstance had taken place at Unst, at which station however the unfavourable weather prevented the commencement of my observations, until the acceleration had nearly attained its maximum. To satisfy myself on this point, I took down the clock on the 13th August, and having carefully cleaned it, began a new series of observations, which are given at large in the Appendix, and from which the following tables and results are derived:

Transits observed at PORTSOY. 2d Series.

Stars.	August 13.	August 14.	August 15.	August 16.	August 17.	August 18.	August 19.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
The Sun	0.11.47.72	—	0.12.49.27	0.13.19.72	0.13.49.88	—	0.14.49.28
Arcturus	4.48.35.44	—	4.42. 6.66	4.38.53.17	—	4.32.25.84	—
α Ophiuchi	8. 7.17.18	8. 4. 2.83	8. 0.48.78	7.57.34.88	7.54.21.23	—	—
γ Ophiuchi	—	8.26.31.46	8.23.16.94	—	8.16.49.55	8.13.36.30	—
ϵ Serpentis	—	8.49.21.19	8.46. 7. 0	8.42.53.09	8.39.39.77	8.36.26.36	—
α Lyrae	—	9. 8.11.68	9. 4.57.59	9. 1.43.81	8.58.30.19	8.55.16.39	—
μ Aquilæ	—	10. 2.29.88	—	—	9.52.48.21	—	—
α Aquilæ	—	10.19. 9.84	—	—	10. 9.28.18	—	—

Rate of the clock at PORTROY. 2d Series. (Gaining.)

Sizes.	From 13 to 14.	From 13 to 15.	From 13 to 16.	From 13 to 17.	From 13 to 18.	From 13 to 19.	From 14 to 15.	From 14 to 16.	From 14 to 17.	From 14 to 18.	From 15 to 16.	From 15 to 17.	From 15 to 18.	From 15 to 19.	From 16 to 17.	From 16 to 18.	From 16 to 19.	From 17 to 18.	From 17 to 19.	From 18 to 19.
	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.
The Sun	—	41.62	41.80	41.91	42.05	42.15	42.05	42.18	42.10	42.10	42.18	42.15	42.20	42.41	42.45	42.26	42.36	42.55	—	42.70
Arcturus	—	41.63	41.93	—	—	—	—	—	—	—	—	—	—	42.41	—	—	—	—	—	—
α Ophiuchi	41.67	41.82	41.92	42.03	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
γ Ophiuchi	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
γ Serpentis	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
α Lyrae	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
α Aquilæ	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
α Aquilæ	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Mean by the Stars	41.67	41.72	41.92	42.03	42.02	42.14	42.25	42.25	42.33	42.41	—	—	—	—	—	—	—	—	—	—
Mean by the Sun	—	41.62	41.80	41.91	—	—	—	42.18	—	—	—	—	42.20	—	42.45	42.26	—	42.55	—	42.70

Vibrations of the Pendulum at PORTSMOUTH, 2d SERIES. The clock making 86442.18 Vibrations in a mean solar day						
Date.		Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours at 62 degrees.
Aug. 13	A. M.	—	—	—	—	—
	P. M.	30.25	61.9	86081.04	0.04	86081.08
14	A. M.	30.25	60.3	86081.11	0.72	86080.39
	P. M.	30.27	62.4	86080.19	0.17	86080.36
15	A. M.	30.25	60.1	86080.85	0.80	86080.05
	P. M.	30.25	61.6	86080.13	0.17	86079.96
16	A. M.	30.18	58.4	86081.19	1.52	86079.67
	P. M.	30.17	60.9	86080.26	0.47	86079.79
17	A. M.	30.15	59.8	86080.60	0.91	86079.69
	P. M.	30.16	61.2	86080.11	0.34	86079.77
18	A. M.	30.14	58.4	86080.79	1.52	86079.27
	P. M.	30.14	60.2	86080.18	0.76	86079.42
19	A. M.	30.10	57.4	86080.85	1.95	86078.90
	P. M.	—	—	—	—	—
Mean		30.19	60.2			86079.84

It appears from the above Table, as well as by the comparisons of the clock with the chronometer, that the rate of the clock had been sufficiently uniform to render any correction on this head unnecessary; in the following Table therefore we have the number of vibrations made by the pendulum in a mean solar day.

By the Stars. 2d Series. PORTSOY.				
From	To	Correct Vibrations in a mean solar day.	No. of stars observed.	Inter. of Transits.
14 A. M.	14 P. M.	86079,86	1	1
14 A. M.	15 P. M.	86079,73	2	2
14 A. M.	16 P. M.	86079,78	2	3
14 A. M.	17 P. M.	86079,81	1	4
14 A. M.	18 P. M.	86079,76	1	5
15 A. M.	15 P. M.	86079,63	4	1
15 A. M.	16 P. M.	86079,71	3	2
15 A. M.	17 P. M.	86079,78	6	3
15 A. M.	18 P. M.	86079,77	3	4
16 A. M.	16 P. M.	86079,80	4	1
16 A. M.	17 P. M.	86079,87	4	2
16 A. M.	18 P. M.	86079,83	4	3
17 A. M.	17 P. M.	86080,03	3	1
17 A. M.	18 P. M.	86079,79	3	2
18 A. M.	18 P. M.	86079,69	3	1
By the Sun. 2d. Series.				
13 P. M.	15 A. M.	86079,89	2	2
13 P. M.	16 A. M.	86079,86	2	3
13 P. M.	17 A. M.	86079,84	2	4
13 P. M.	19 A. M.	86079,85	2	6
15 P. M.	16 A. M.	86079,78	2	1
15 P. M.	17 A. M.	86079,79	2	2
15 P. M.	19 A. M.	86079,83	2	4
16 P. M.	17 A. M.	86079,81	2	1
16 P. M.	19 A. M.	86079,84	2	3
17 P. M.	19 A. M.	86079,86	2	2

Employing the numbers of stars observed, and the intervals of the transits, as before, we obtain 86079,78 vibrations by the observations of the stars, and 86079,84 by those of the sun; and the sums of the factors being 96 and 36, we have 86079,80 for the final mean number of vibrations in 24 hours.

The mean height of the barometer was 30,19 inches, and that of the thermometer 60°, 2, hence the correction for the buoyancy of the atmosphere is 6,07.

This correction, together with 0.23 (the correction for the height above the sea) being added to the mean number of vibrations, we have 86086.10 for the number of vibrations which would be made in a mean solar day, in vacuo, and at the level of the sea.

The difference between this result and that of the first series of experiments made under the most unfavourable circumstances of acceleration in the rate of the clock being only 0.09, affords it is presumed a most satisfactory proof that no very important error is to be dreaded from this source in the observations at Unst.

Operations at Leith Fort.

Having completed the requisite observations for the latitude of my station, and for connecting it with Cowhythe, I quitted Portsoy for Edinburgh on the 20th August, leaving the instruments and party to come by sea.

Leith Fort was my next station, and here, as I could procure no lodgings in the neighbourhood, an officer of the Royal Artillery most kindly relinquished to me his quarters in the barracks. The Cherokee arrived on the 28th, and the instruments were landed the same evening.

On my first arrival at Edinburgh to embark for Unst, I had been introduced to Sir HOWARD ELPHINSTONE, the chief engineer of the station, and received from him the assurance of every assistance in my experiments, which his department could furnish. Though to my regret he was now absent on duty, I was promptly supplied with such materials and artificers as were necessary, and on the 29th August my apparatus was firmly put up in one of the public store rooms of the Fort, which was excellently adapted to the purpose, and the

transit instrument placed on a massy stone foundation, erected for it on the ramparts.

On the 31st of August I commenced my observations, the results of which are given in the following Tables, and on the evening of the 7th of September, the transits of the same stars were again observed, but unfortunately the lamp which was attached to the meridian mark, for adjusting the transit instrument by night, not having been properly placed, these observations were of necessity rejected.

Transits observed at LEITH FORT. 1st Series.

Stars.	August 31.	September 2.	September 4.	September 5.	September 6.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
The Sun .	—	0. 9.18,05	—	0. 9.41,66	0. 9.51,50
♑ Capricorni	9.49.41,04	9.42.40,21	9.35.42,16	—	—
♒ Aquarii	—	10. 1.56,92	9.54.59,09	—	—
♓ Equulei	10.37.46,18	10.30.45,31	10.23.47,39	—	—
♊ Aquarii	10.52.59,93	10.45.59,62	10.39. 1,37	—	—
♈ Pegasi	11. 6.12,53	—	10.52.13,94	—	—
♉ Aquarii	11.24.50,38	11.17.49,91	11.10.51,95	—	—
♊ Aquarii	11.43. 8,89	—	11.29.10,60	—	—
♋ Aquarii	11.59.11,46	—	11.45.13,07	—	—
♌ Aquarii	—	11.55.13,67	—	—	—
♍ Pegasi	—	12. 1.24,05	11.54.26,26	—	—

From these transits the following table was computed.

Rate of the clock at LEITH FORT. 1st Series. (Gaming.)						
Stars.	From August 31, to Sept. 2.	From August 31, to Sept. 4.	From Sept. 2, to Sept. 4.	From Sept. 2, to 3.	From Sept. 2, to 6.	From Sept. 3, to 6.
The Sun .	—	—	—	27,04	27,69	29,64
α Capricorni	25,56	26,26	26,95	—	—	—
ι Aquarii .	—	—	27,07	—	—	—
α Equulei .	25,55	26,28	27,02	—	—	—
β Aquarii .	25,83	26,34	26,85	—	—	—
ι Pegasi .	—	26,33	—	—	—	—
γ Aquarii .	25,75	26,37	27,00	—	—	—
γ Aquarii .	—	26,41	—	—	—	—
ι Aquarii .	—	26,38	—	—	—	—
η Aquarii .	—	—	—	—	—	—
ξ Pegasi .	—	—	27,09	—	—	—
Mean by the Stars }	25,67	26,34	27,00	—	—	—
Mean by the Sun }	—	—	—	27,04	27,69	29,64

The steeple of Leith church, being very conveniently situated for the purpose, I was anxious to ascertain with what degree of precision the rate of the clock might be obtained, by observing the disappearance of stars behind the steeple, a method which I understand was employed by M. BIOT, in his late ~~laborious~~ experiments on the length of the pendulum, and which seems capable of great accuracy. For this purpose I used a powerful achromatic telescope, with which I was favoured by Mr. JARDINE from the observatory. The telescope was placed so as to rest against the door way of the room which contained the clock, and was directed towards the side of the steeple. On the evening of the 30th August, I obtained observations of the time of the disappearance of several stars, and on the 6th of September, two of these stars were again observed, but the rest were not visible. By these stars,

the rate of the clock appeared to be 26^s.85; which rate as it was deduced from the longest interval, has been used in computing the following Table.

Vibrations of the Pendulum at LEITH FORT. 1st. Series. The clock making 86426.85 vibrations in a mean solar day.						
Date.		Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours at 62 degrees.
Aug. 31	A. M.	29.95	56.6	86078.34	2.28	86076.06
	P. M.	29.85	58.9	86077.50	1.30	86076.20
Sept. 1	A. M.	29.55	58.7	86076.08	1.40	86074.68
	P. M.	29.49	60.1	86075.72	0.80	86074.92
2	A. M.	29.58	58.4	86075.14	1.52	86073.62
	P. M.	29.68	59.9	86074.55	0.89	86073.66
3	A. M.	29.95	57.4	86075.13	1.95	86073.18
	P. M.	29.97	59.7	86074.13	0.97	86073.16
4	A. M.	29.78	59.5	86074.13	1.06	86073.07
	P. M.	29.76	61.9	86073.16	0.04	86073.12
5	A. M.	29.85	60.3	86072.43	0.72	86071.71
	P. M.	29.83	62.1	86071.57	+0.04	86071.61
6	A. M.	29.60	59.9	86070.85	0.89	86069.96
	P. M.	29.62	61.4	86070.33	0.25	86070.08
Mean		29.75	59.6			86073.21

By the above Table we may perceive, that though the clock had been cleaned so recently, its rate had notwithstanding increased in seven days, about six seconds, or 0.85 in every 24 hours. On account of this acceleration it becomes necessary to apply a correction, in the manner which has been before explained, in order to obtain the true number of vibrations made by the pendulum in a mean solar day. The results are contained in the following Table.

By the Stars. 1st. Series. LISTED FOR 1.							
From	To	Computed Vibrations in a mean solar day.	Mean of Time in B. or A. com.	Correc- tion.	Corrected vibra- tion, in a mean solar day.	No. of stars observed.	Interval of Time.
			h. m.				
1 A. M.	2 P. M.	86073,04	B. 1.33	+ ,05	86073,09	1	2
1 A. M.	4 P. M.	86073,16	B. 0.28	+ ,02	86073,18	7	4
3 A. M.	4 P. M.	86073,28	B. 0.27	+ ,02	86073,30	6	7
By disappearance of stars behind Leith steeple.		86073,21	A. 0.33	— ,02	86073,19	7	7
By the Sun. 1st. Series.							
2 P. M.	5 A. M.	86073,17	A. 0.57	— 0,1	86073,14	2	3
2 P. M.	6 A. M.	86073,28	A. 1. 9	— 0,3	86073,24	2	4
5 P. M.	6 A. M.	86073,57	A. 0.51	— 0,3	86073,54	2	1

Using the number of stars observed and the intervals of the transits, as before, to obtain a mean, we have 86073,19 vibrations by the stars, and 86073,23 by the sun, and the sums of the factors, being 62 and 16, we obtain 86073,20 for the final mean number of vibrations in 24 hours.

The mean height of the barometer was 29,75 inches, and the mean temperature 59°,6. The correction for the buoyancy of the atmosphere is therefore 5,99.

The height of the pendulum above low water, was found by levelling to be 68 feet, whence we have $0,28 \times 1000 = 0,18$ for the correction due to this elevation.

These corrections being applied, we obtain 86079,37 for the number of vibrations made by the pendulum in a mean solar day in vacuo, and at the level of the sea.

The clock was now taken down to be cleaned, as I had resolved to go through a new series of observations. On examining the oil, it was found to all appearance as pure as

when first applied, and I can in no way account for the acceleration in the rate of the clock, but by supposing, that whilst it was at rest, the external surface of the oil had become thickened by some action of the sea air upon it. This would of course occasion the rate to be less, on the clock being first put up, and a gradual acceleration would afterwards take place as the thick coat of the oil became blended with the more fluid particles beneath. These remarks may perhaps warrant the important inference, that no reliance whatever can be placed on results obtained by means of a pendulum attached to a clock, and that until oil can be banished from chronometers, and the maintaining power be such as to be equal under all circumstances, we may spare ourselves the trouble of attending to other sources of error.

The clock being cleaned, the observations were made and the results deduced which are contained in the following Tables.

Transits observed at LEITH FORT. 2d Series.				
Stars.	September 8.	September 10.	September 12.	September 14.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.
α Equulei	10. 2.39.48	9.55.54.46	9.49.11.23	9.42.28.05
β Aquarii	10.17.53.83	10.11. 8.88	10. 4.25.60	—
γ Pegasi	10.31. 6.30	10.24.21.36	—	10.10.54.92
δ Aquarii	10.49.44.22	—	10.36.16.20	10.29.32.78
ϵ Pegasi	11.14.25.06	11. 7.39.80	11. 0.56.53	10.54.13.42
ζ Aquarii	11.24. 5.62	11.17.20.66	11.10.37.38	11. 3.54.32
η Pegasi	11.28. 6.69	11.21.21.70	11.14.38.28	11. 7.55.24
θ Pegasi	11.33.18.73	11.26.33.72	11.19.50.34	11.13. 7.40
α Pegasi	11.51.21.57	11.44.36.50	11.37.53.33	11.31.10.33

Rate of the clock at LEITH FORT. 2d. Series. (Gump.)						
Stars.	From September 8 to 10.	From 8 to 12.	From 8 to 14.	From 10 to 12.	From 10 to 14.	From 12 to 14.
	S.	S.	S.	S.	S.	S.
α Equulei	33.49	33.94	34.10	34.39	34.40	34.41
β Aquarii	33.53	33.94	—	34.36	—	—
γ Pegasi	33.53	—	34.10	—	34.39	—
δ Aquarii	—	33.99	34.09	—	—	34.29
ϵ Pegasi	33.37	33.87	34.06	34.37	34.41	34.45
ζ Aquarii	33.49	33.93	34.11	34.36	34.41	34.47
η Pegasi	33.51	33.90	34.09	34.29	34.39	34.48
θ Pegasi	33.50	33.90	34.11	34.31	34.42	34.53
α Pegasi	33.47	33.94	34.13	34.42	34.46	34.50
Mean	33.49	33.93	34.10	34.36	34.41	34.45

Vibrations of the Pendulum at LEITH FORT. 2d Series. The clock making 86434.10 vibrations in a mean solar day.						
Date.		Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours at 62 degrees.
Sept. 9	A. M.	29.90	54°.2	86077.10	3.30	86073.80
	P. M.	29.95	55.6	86076.63	2.71	86073.92
10	A. M.	29.94	52.4	86077.45	4.06	86073.39
	P. M.	29.91	54.2	86076.98	3.30	86073.68
12	A. M.	29.92	51.5	86077.16	4.44	86072.72
	P. M.	29.95	53.8	86076.71	3.68	86073.03
13	A. M.	30.14	53.1	86076.64	3.77	86072.87
	P. M.	30.14	54.2	86076.22	3.30	86072.92
13	A. M.	30.28	54.0	86076.05	3.38	86072.67
	P. M.	30.24	55.9	86075.40	2.58	86072.82
14	A. M.	29.89	56.4	86075.35	2.37	86072.98
	P. M.	29.85	57.1	86074.86	2.07	86072.79
Mean		30.01	54.3			86073.13

We may perceive from the above Table, that the rate of the clock had encreased about a second in six days; the error however affecting the final number of vibrations of the pendulum, in consequence of this, is too small to need correction.

By the Stars. LEITH FORT. 2d Series.				
From	To	Correct Vibrations in a Mean solar day.	No. of stars observed.	Interv. of Transits.
9 A. M.	10 P. M.	86073,09	8	2
9 A. M.	12 P. M.	86073,12	8	4
9 A. M.	14 P. M.	86073,13	8	6
11 A. M.	12 P. M.	86073,14	7	2
11 A. M.	14 P. M.	86073,16	7	4
13 A. M.	14 P. M.	86073,17	7	2

Using the number of stars observed, and the intervals between the transits as before, we have 86073,18 for the number of vibrations in 24 hours.

The barometer being at 30,01 inches, and the thermometer at 54°,3 the correction for the buoyancy of the atmosphere is 6,11.

This correction, together with 0,18, the correction for the height above the sea, being applied, we obtain 86079,42 for the number of vibrations made by the pendulum in vacuo, as deduced from the second series, from which the result of the first series differs 0,05 of a vibration. The mean of both is to be preferred.

Operations at Clifton.

On the 17th of September I left Edinburgh, and proceeded to Clifton in Yorkshire; at which place my instruments and party arrived on the 28th. Here I was so fortunate as to meet with a vacant house in the village, perfectly suited to my purpose, belonging to Mr. MILWARD, who is also proprietor of the field in which is the station of the Trigonometrical Survey. Previous to the commencement of my experiments, the clock was carefully cleaned. The observations were then made, and

372 *Capt. KATER's experiments for determining the variation*

the results deduced which are contained in the following Tables.

Transits observed at CLIFTON.					
Stars.	October 2.	October 3.	October 4.	October 5.	October 6.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
The Sun	—	11.49. 6. 5	11.48. 8. 64	11.47.40.36	11.46.15.38
♈ Aquilæ	6.46.47.15	6.42.40.48	6.34.27.35	6.30.20.82	6.22. 8.92
♈ Aquilæ	6.58.27.38	6.54.20.37	6.46. 7.22	6.42. 0.57	—
♈ Aquilæ	7.18.23.75	7.14.16.85	7. 6. 3.75	7. 1.57.15	6.53.14.63
♈ Aquarii	—	7.50. 4.50	7.41.51.28	7.37.44.75	7.29.32.8
♈ Capricorni	—	8. 6.13.77	—	—	—
♈ Equulei	8.23. 2.35	8.18.55.28	—	8. 6.35.35	7.58.23.28
♈ Capricorni	8.43. 5.98	8.38.58.87	—	8.26.39.37	8.18.27.2
♈ Aquarii	9.12.35.95	9. 8.29.15	9. 0.15.68	8.56. 9.23	8.47.57.07
♈ Aquarii	9.28.22.38	9.24.15.47	9.16. 2.27	9.11.55.72	—
♈ Aquarii	—	9.37.58. 1	9.29.44.92	9.25.38.43	9.17.26.37

Rate of the clock at CLIFTON. (Losing.)										
Stars.	From Oct. 2, to 3.	From 2 to 3.	From 3 to 4.	From 4 to 5.	From 5 to 6.	From 6 to 7.	From 7 to 8.	From 8 to 9.	From 9 to 10.	From 10 to 11.
	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
The Sun	—	—	—	—	10.78	10.75	10.62	10.68	10.52	10.44
♈ Aquilæ	11.09	10.82	10.78	10.54	10.68	10.67	10.43	10.65	10.26	10.07
♈ Aquilæ	11.13	10.84	10.82	—	10.69	10.72	—	10.77	—	—
♈ Aquilæ	11.03	10.79	10.77	10.64	10.67	10.69	10.56	10.72	10.49	10.38
♈ Aquarii	—	—	—	—	10.73	10.70	10.46	10.65	10.28	10.09
♈ Capricorni	—	—	—	—	—	—	—	—	—	—
♈ Equulei	11.19	—	10.87	10.63	—	10.76	10.52	—	—	10.15
♈ Capricorni	11.23	—	10.77	10.58	—	10.62	10.45	—	—	10.20
♈ Aquarii	10.92	10.88	10.80	10.60	10.85	10.76	10.54	10.57	10.32	10.20
♈ Aquarii	11.03	10.82	10.78	—	10.72	10.70	—	10.67	—	—
♈ Aquarii	—	—	—	—	10.71	10.68	10.47	10.61	10.30	10.15
Mean by the Stars }	11.09	10.83	10.80	10.60	10.78	10.70	10.49	10.66	10.33	10.18
Mean by the Sun }	—	—	—	—	10.78	10.75	10.62	10.68	10.52	10.44

Vibrations of the Pendulum at CLIFTON. The clock making 86389.40 vibrations in a mean solar day.						
Date.		Barometer	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours, at at 62 degrees.
Oct.						
3	A. M.	29.22	57.4	86064.52	1.05	86062.57
	P. M.	29.20	58.2	86063.92	1.61	86062.31
4	A. M.	29.18	57.2	86064.44	2.03	86062.41
	P. M.	29.13	57.2	86064.18	2.03	86062.15
5	A. M.	29.10	55.1	86065.26	2.92	86062.34
	P. M.	29.08	55.7	86064.93	2.67	86062.26
6	A. M.	29.01	53.4	86065.75	3.64	86062.11
	P. M.	29.10	54.5	86065.08	3.17	86061.91
7	A. M.	29.30	52.9	86065.47	3.85	86061.62
	P. M.	29.33	53.7	86065.25	3.51	86061.74
8	A. M.	29.52	52.2	86065.36	4.15	86061.21
	P. M.	29.57	52.9	86065.08	3.85	86061.23
Mean		29.23	55.0			86061.99

From the preceding Tables, the following vibrations in a mean solar day were computed.

By the Stars. CLIFTON.				
From	To	Correct Vibrations in a mean solar day.	No. of Stars observed	Interv. of Transits.
3 A. M.	3 P. M.	86061.95	7	1
3 A. M.	5 P. M.	86062.11	5	3
3 A. M.	6 P. M.	86062.06	7	4
3 A. M.	8 P. M.	86061.99	5	6
4 A. M.	5 P. M.	86062.17	7	2
4 A. M.	6 P. M.	86062.10	9	3
4 A. M.	8 P. M.	86062.01	7	5
6 A. M.	6 P. M.	86061.95	7	1
6 A. M.	8 P. M.	86061.91	5	3
7 A. M.	8 P. M.	86061.87	7	2
By the Sun.				
3 P. M.	5 A. M.	86063.12	2	2
3 P. M.	6 A. M.	86062.11	2	3
3 P. M.	8 A. M.	86061.99	2	5
5 P. M.	6 A. M.	86062.10	2	1
5 P. M.	8 A. M.	86061.89	2	3
6 P. M.	8 A. M.	86061.78	2	2

The number of stars observed, and the intervals between the transits being employed as before to obtain a mean, we have 86062,02 vibrations by the stars, and 86061,99 by the sun, whence we obtain 86062,01 for the final mean number of vibrations in 24 hours.

The height of the barometer being 29,23 inches, and the thermometer 55°,0 the resulting correction for the buoyancy of the atmosphere is 5,94.

The height of Clifton Beacon, above the level of the sea is stated in the "Account of the Trigonometrical Survey" to be 417 feet; and by levelling, the pendulum was found to be 78 feet below Clifton Beacon, the height of the pendulum therefore above the level of the sea was 339 feet, the correction for which is $1,40 \times \frac{62}{100} = 0,95$.

Applying these corrections, we obtain 86068,90 for the number of vibrations at Clifton, in a mean solar day, in vacuo and at the level of the sea.

Operations at Arbury Hill.

On the 18th of October I left Clifton, having previously made some important observations for the latitude, which will be detailed in the proper place, and proceeded to Arbury Hill, where my party and instruments arrived on the 15th. Here I procured accommodations at a house belonging to Mr. GOSAGE, situated on the side of an eminence, to the south of Arbury Hill. The season was now so far advanced, and the weather in consequence so variable, that it was not until the 21st that I was able to commence my observations. These though few in number, were made with such minute precautions, and under such favourable circumstances, as to be perfectly satisfactory to me. The following Tables contain the results.

in the length of the pendulum vibrating seconds.

875

Transits observed at ARBURY HILL.			
Stars.	October 21.	October 25.	October 26.
	h. m. s.	h. m. s.	h. m. s.
The Sun	11.44.28.39	—	11.43.17.93
σ Aquilæ	5.31.39.53	5.15.30.75	5.11.29.10
α Aquilæ	5.43.19.17	5.27.10.42	5.23. 8.78
θ Aquilæ	6. 3.16.55	5.47. 7.78	5.43. 6.05

Rate of the clock at ARBURY HILL. (Losing.)			
Stars.	From 21 to 25.	From 21 to 26.	From 25 to 26.
	s.	s.	s.
The Sun	—	6.23	—
σ Aquilæ	6.30	6.20	5.76
α Aquilæ	6.30	6.19	5.75
θ Aquilæ	6.30	6.21	5.84
Mean by the Stars	6.30	6.20	5.78
Mean by the Sun	—	6.23	—

Vibrations of the Pendulum at ARBURY HILL.						
The clock making 86393.80 vibrations in a mean solar day.						
Date.		Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours, at 62 degrees.
Oct.						
21	A. M.	—	—	—	—	—
	P. M.	29.65	56.7	86059.25	2.24	86057.01
22	A. M.	29.52	54.2	86060.66	3.30	86057.36
	P. M.	29.50	54.4	86060.52	3.22	86057.30
23	A. M.	29.50	52.8	86061.07	3.89	86057.18
	P. M.	29.52	53.2	86060.88	3.72	86057.16
24	A. M.	29.57	50.8	86061.40	4.74	86056.66
	P. M.	29.55	50.6	86061.28	4.82	86056.46
25	A. M.	29.56	50.9	86061.40	4.70	86056.70
	P. M.	29.54	52.3	86061.00	4.10	86056.90
26	A. M.	29.55	52.2	86060.63	4.15	86056.48
	P. M.	29.55	53.7	86060.12	3.51	86056.61
Mean		29.55	52.9			86056.88

From the preceding Tables were deduced the following vibrations in a mean solar day.

By the Stars. ARBURY HILL				
From	To	Correct Vibrations in a mean solar day.	No. of Stars observed	Interv. of Transits
22 A. M.	25 P. M.	86056,86	3	4
22 A. M.	26 P. M.	86056,88	3	5
26 A. M.	26 P. M.	86056,96	3	1
By the Sun.				
21 P. M.	26 A. M.	86056,89	2	5

From the number of stars observed, and the intervals of the transits, we derive 86056,88 for the mean by the stars, 86056 89 by the sun, and 86056,88 for the final mean number of vibrations in 24 hours.

The barometer being at 29,55 inches, and the thermometer at 52°,9 we have 6,04 for the correction on account of the buoyancy of the atmosphere.

The angle of elevation of the top of the tent on Arbury Hill, taken by the repeating circle from the station where the clock was placed, was found to be 1°.28'.21",4; and as it will appear in the Appendix, that the distance from the station on Arbury Hill to the clock, was 3048 feet, we have 78 feet very nearly for the elevation of the top of the tent above the pendulum. The elevation of Arbury Hill above the sea, as determined by the Trigonometrical Survey, is 804 feet, from which deducting 67 feet, (the height of the tent being 11 feet,) we obtain 737 feet for the elevation of the pendulum above the

level of the sea, the correction for which is $3,04 \times \frac{7}{16} = 2,13$. These corrections being applied, we have 86065,05 for the number of vibrations which would be made by the pendulum in a mean solar day in vacuo and at the level of the sea.

On leaving Arbury Hill, I hastened to Dunnose in the Isle of Wight, anxious to complete my experiments before the winter; but on arriving there, I found the weather so bad, that after a short stay I was reluctantly obliged to postpone my observations at that station until the following spring.

Operations at London.

Before I left London in June, I took four series of vibrations of the pendulum at a high temperature, at Mr. BROWNE's house in Portland Place; chiefly with a view to afford me the means of checking my expansion of the pendulum by a comparison with other series of vibrations, which I purposed to observe at a low temperature on my return, and also to enable me to form some idea of the acceleration, when I should arrive at Unst. For the rate of the clock I am indebted to the observations of Mr. BROWNE. The results are contained in the following Table.

Vibrations of the Pendulum at LONDON.—1st Series.					
Date, 1818.	Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Correct Vibrations in a mean solar day at 62 degrees.
June					
13	29,90	71,6	86051,32	4,06	86055,38
14	30,00	70,1	86051,90	3,43	86055,33
15	30,05	69,9	86051,99	3,34	86055,33
16	29,95	70,5	86051,82	3,60	86055,42
Mean	29,98	70,5			86055,36

The barometer being at 29,98 inches, and the thermometer
MDCCCXIX. 3 D

378 *Capt. KATER's experiments for determining the variation*

at $70^{\circ},5$ the correction for the buoyancy of the atmosphere is 5,91.

The height of the pendulum above the level of the sea was 83 feet, the correction for which is $0,34 \times \frac{66}{100} = 0,22$.

These corrections being applied, we have 86061,49 vibrations in a mean solar day, at the temperature of 62° in vacuo, and at the level of the sea.

Various causes prevented me from repeating my experiments in London, until the month of March, when the following results were obtained, the observations on which they are founded being detailed in the Appendix.

Vibrations of the Pendulum at LONDON.—2nd Series.					
Date. 1810.	Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Correct Vibrations in a mean solar day, in vacuo at 62°
March					
8	30,10	50,0	86060,12	5,08	86055,04
9	30,10	50,1	86060,21	5,04	86055,18
15	30,14	51,8	86059,41	4,32	86055,09
16	30,00	52,7	86058,98	3,93	86055,05
17	30,10	53,5	86058,92	3,60	86055,32
18	30,21	52,8	86058,93	3,89	86055,04
Mean	30,11	51,8			86055,12

The correction for the buoyancy of the atmosphere is 6,18, and that for the height above the level of the sea, 0,22. We have therefore 86061,52 for the number of vibrations at 62° in vacuo, and at the level of the sea.

So very near an agreement with my former observations, after an allowance for a difference of temperature amounting to $18^{\circ},7$ I could scarcely have dared to hope for, and it afforded me a most satisfactory assurance, not only that the

knife edge of the pendulum had suffered no injury from use, but that my allowance for expansion was correct, a circumstance of the greatest importance to the truth of my results, and respecting which there might have been most reason to apprehend error.

Operations at the Isle of Wight.

On the 8th May 1819, I again left London for the Isle of Wight. Dunnose, the most southern station of the meridional arc of the Trigonometrical Survey, is marked by an iron gun, sunk in the ground on the summit of a hill near the village of Shanklin, a little to the north of a signal post.* The nearest house to this station is Shanklin Farm, in the occupation of Mr. JOLLIFFE, from whom and from the proprietor, the Rev. Mr. WHITE, I most readily received permission to make use of a summer house, well suited to the purpose, for my experiments.

The observations made at this station are detailed in the Appendix. The weather was very favourable after the 12th; and though before that period I was not able to obtain the transit of more than one star and of the sun, these observations were satisfactory. The results are contained in the following Tables.

Transits observed at SHANKLIN FARM.							
Stars.	May 10.	May 11.	May 12.	May 13.	May 14.	May 15.	May 16.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
The Sun	—	0.0.49.49	—	0.0.26.89	0.0.16.44	0.0.6.51	11.59.57.71
Regulus	6.52.37.32	—	—	6.40.21.23	—	6.32.10.35	6.28.5.65
♌	—	—	9.11.21.26	—	—	—	8.55.0.74
♍ Virginis	—	—	9.38.24.16	—	—	—	9.22.2.83
♍ Virginis	—	—	10.0.49.30	—	—	—	9.44.28.08
♋ Bootæ	—	—	10.23.46.29	—	—	—	10.7.25.28
♋ Bootæ	—	—	10.31.9.37	—	—	—	10.14.38.58
♋ Arcturus	—	—	10.52.26.25	—	—	—	10.36.5.44

* The height on which the station is situated, is properly called *Shanklin Down*; Dunnose is the next projecting point to the southward.

	From 10 to 12.	From 10 to 13.	From 10 to 14.	From 11 to 15.	From 11 to 16.	From 11 to 17.	From 12 to 18.	From 12 to 19.	From 13 to 20.	From 14 to 21.	From 15 to 22.	From 16 to 23.	From 17 to 24.	From 18 to 25.	From 19 to 26.	From 20 to 27.	From 21 to 28.	From 22 to 29.	From 23 to 30.	From 24 to 31.	From 25 to 32.	From 26 to 33.	From 27 to 34.	From 28 to 35.	From 29 to 36.	From 30 to 37.	From 31 to 38.	From 32 to 39.	From 33 to 40.	From 34 to 41.	From 35 to 42.	From 36 to 43.	From 37 to 44.	From 38 to 45.	From 39 to 46.	From 40 to 47.	From 41 to 48.	From 42 to 49.	From 43 to 50.	From 44 to 51.	From 45 to 52.	From 46 to 53.	From 47 to 54.	From 48 to 55.	From 49 to 56.	From 50 to 57.	From 51 to 58.	From 52 to 59.	From 53 to 60.	From 54 to 61.	From 55 to 62.	From 56 to 63.	From 57 to 64.	From 58 to 65.	From 59 to 66.	From 60 to 67.	From 61 to 68.	From 62 to 69.	From 63 to 70.	From 64 to 71.	From 65 to 72.	From 66 to 73.	From 67 to 74.	From 68 to 75.	From 69 to 76.	From 70 to 77.	From 71 to 78.	From 72 to 79.	From 73 to 80.	From 74 to 81.	From 75 to 82.	From 76 to 83.	From 77 to 84.	From 78 to 85.	From 79 to 86.	From 80 to 87.	From 81 to 88.	From 82 to 89.	From 83 to 90.	From 84 to 91.	From 85 to 92.	From 86 to 93.	From 87 to 94.	From 88 to 95.	From 89 to 96.	From 90 to 97.	From 91 to 98.	From 92 to 99.	From 93 to 100.	From 94 to 101.	From 95 to 102.	From 96 to 103.	From 97 to 104.	From 98 to 105.	From 99 to 106.	From 100 to 107.	From 101 to 108.	From 102 to 109.	From 103 to 110.	From 104 to 111.	From 105 to 112.	From 106 to 113.	From 107 to 114.	From 108 to 115.	From 109 to 116.	From 110 to 117.	From 111 to 118.	From 112 to 119.	From 113 to 120.	From 114 to 121.	From 115 to 122.	From 116 to 123.	From 117 to 124.	From 118 to 125.	From 119 to 126.	From 120 to 127.	From 121 to 128.	From 122 to 129.	From 123 to 130.	From 124 to 131.	From 125 to 132.	From 126 to 133.	From 127 to 134.	From 128 to 135.	From 129 to 136.	From 130 to 137.	From 131 to 138.	From 132 to 139.	From 133 to 140.	From 134 to 141.	From 135 to 142.	From 136 to 143.	From 137 to 144.	From 138 to 145.	From 139 to 146.	From 140 to 147.	From 141 to 148.	From 142 to 149.	From 143 to 150.	From 144 to 151.	From 145 to 152.	From 146 to 153.	From 147 to 154.	From 148 to 155.	From 149 to 156.	From 150 to 157.	From 151 to 158.	From 152 to 159.	From 153 to 160.	From 154 to 161.	From 155 to 162.	From 156 to 163.	From 157 to 164.	From 158 to 165.	From 159 to 166.	From 160 to 167.	From 161 to 168.	From 162 to 169.	From 163 to 170.	From 164 to 171.	From 165 to 172.	From 166 to 173.	From 167 to 174.	From 168 to 175.	From 169 to 176.	From 170 to 177.	From 171 to 178.	From 172 to 179.	From 173 to 180.	From 174 to 181.	From 175 to 182.	From 176 to 183.	From 177 to 184.	From 178 to 185.	From 179 to 186.	From 180 to 187.	From 181 to 188.	From 182 to 189.	From 183 to 190.	From 184 to 191.	From 185 to 192.	From 186 to 193.	From 187 to 194.	From 188 to 195.	From 189 to 196.	From 190 to 197.	From 191 to 198.	From 192 to 199.	From 193 to 200.	From 194 to 201.	From 195 to 202.	From 196 to 203.	From 197 to 204.	From 198 to 205.	From 199 to 206.	From 200 to 207.	From 201 to 208.	From 202 to 209.	From 203 to 210.	From 204 to 211.	From 205 to 212.	From 206 to 213.	From 207 to 214.	From 208 to 215.	From 209 to 216.	From 210 to 217.	From 211 to 218.	From 212 to 219.	From 213 to 220.	From 214 to 221.	From 215 to 222.	From 216 to 223.	From 217 to 224.	From 218 to 225.	From 219 to 226.	From 220 to 227.	From 221 to 228.	From 222 to 229.	From 223 to 230.	From 224 to 231.	From 225 to 232.	From 226 to 233.	From 227 to 234.	From 228 to 235.	From 229 to 236.	From 230 to 237.	From 231 to 238.	From 232 to 239.	From 233 to 240.	From 234 to 241.	From 235 to 242.	From 236 to 243.	From 237 to 244.	From 238 to 245.	From 239 to 246.	From 240 to 247.	From 241 to 248.	From 242 to 249.	From 243 to 250.	From 244 to 251.	From 245 to 252.	From 246 to 253.	From 247 to 254.	From 248 to 255.	From 249 to 256.	From 250 to 257.	From 251 to 258.	From 252 to 259.	From 253 to 260.	From 254 to 261.	From 255 to 262.	From 256 to 263.	From 257 to 264.	From 258 to 265.	From 259 to 266.	From 260 to 267.	From 261 to 268.	From 262 to 269.	From 263 to 270.	From 264 to 271.	From 265 to 272.	From 266 to 273.	From 267 to 274.	From 268 to 275.	From 269 to 276.	From 270 to 277.	From 27
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Vibrations of the Pendulum at SHANKLIN FARM, The clock making 86390,60 vibrations in a mean solar day.						
Date.		Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours at 62 degrees.
May 11	A. M.	30,17	60,9	86052,14	0,47	86051,67
	P. M.	30,16	61,8	86051,73	0,08	86051,65
	A. M.	30,10	61,0	86051,96	0,42	86051,54
	P. M.	30,09	61,3	86051,85	0,30	86051,55
	A. M.	30,08	60,8	86051,73	0,51	86051,22
	P. M.	30,08	61,0	86051,64	0,42	86051,22
	A. M.	30,14	60,5	86052,14	0,63	86051,51
	P. M.	30,10	60,8	86051,97	0,51	86051,46
	A. M.	30,05	60,9	86051,44	0,47	86050,97
	P. M.	30,05	61,3	86051,28	0,30	86050,98
	A. M.	30,03	60,1	86051,70	0,80	86050,90
	P. M.	30,03	60,7	86051,34	0,55	86050,79
	Mean	30,09	60,9			86051,29

From the preceding tables were deduced the following vibrations in a mean solar day.

By Regulat. SHANKLIN FARM.				
From	To	Correct vibrations in a mean solar day.	No. of stars observed.	Interv. of Trans.
11 A. M.	13 P. M.	86051,39	1	3
11 A. M.	15 P. M.	86051,27	1	5
11 A. M.	16 P. M.	86051,29	1	6
14 A. M.	15 P. M.	86051,17	1	2
14 A. M.	16 P. M.	86051,19	1	3
16 A. M.	16 P. M.	86051,42	1	1
By other Stars.				
13 A. M.	16 P. M.	86051,18	6	4

By the Sun.				
From	To	Correct vibrations in a mean solar day.	No. of stars observed.	Inter. of Transits.
11 P. M.	13 A. M.	86051,54	2	2
11 P. M.	14 A. M.	86051,47	2	3
11 P. M.	15 A. M.	86051,37	2	4
11 P. M.	16 A. M.	86051,36	2	5
13 P. M.	14 A. M.	86051,31	2	1
13 P. M.	15 A. M.	86051,20	2	2
13 P. M.	16 A. M.	86051,24	2	3
14 P. M.	15 A. M.	86051,08	2	1
14 P. M.	16 A. M.	86051,22	2	2
15 P. M.	16 A. M.	86051,34	2	1

The number of stars observed and the intervals between the transits being employed as before to obtain a mean, we have 86051,28 vibrations by Regulus, 86051,18 by the other stars, and 86051,34 by the sun; and the sum of the respective factors being 20, 24, and 48, we obtain 86051,28 for the final mean number of vibrations in 24 hours.

The mean height of the barometer being 30,09 inches, and that of the thermometer 60°,9, the correction for the buoyancy of the atmosphere is 6,09.

It may be seen in the Appendix, that the height of Dunnose above the summer house, deduced from the distance and angle of elevation of the signal post, is 539 feet; and as Dunnose is stated, in the Trigonometrical Survey, to be 792 feet above the level of the sea, this would give 253 feet for the elevation of the pendulum above the sea. But by observations made with a barometer of Sir HARRY ENGLEFIELD'S construction, on three several days, the greatest difference of the results being eight feet, the mean elevation of the summer house above high water mark appeared to be 221 feet; and if 10 feet be allowed for the fall of the tide, we have 231 feet, for the height of the pendulum above low water, differing

from the former result 22 feet. The height of Dunnose above the summer house, was also deduced barometrically, and appeared to be 513 feet, differing from the trigonometrical determination 26 feet in defect. If this difference be attributed to error in the barometer, as is most probably the fact, the proportional error in the elevation of the summer house, determined barometrically, will be 11 feet, and this being added to 231 feet, we have 242 feet for the height of the pendulum above the level of the sea, which is probably within eleven feet of the truth.

The correction due to an elevation of 242 feet, is $0,997 \times \frac{7}{8} = 0,70$; and this, together with the correction for the buoyancy of the atmosphere being added to the number of vibrations before found, we obtain 86058,07 for the number of vibrations which would be made by the pendulum in a mean solar day, in vacuo, and at the level of the sea.

Of the Latitudes and Longitudes of the different Stations.

The daily rate of Mr. BROWNE's chronometer before I left London, was $-0^s,2$ the chronometer being too slow for Greenwich time on the 15th June $1^m.15^s.75$; but this rate, as might have been expected, varied from the motion of the waggon or other causes, so that at Unst, its mean rate was $-1^s,32$, at Portsoy $-1^s,7$, and at Leith $-2^s,42$, which rates are deduced from the column headed "chronometer," in the table of transits given in the Appendix.

The meridian of my station at Leith Fort, passed within 40 feet of that of the observatory on the Calton Hill, the longitude of which Mr. JARDINE, who has the care of the observatory, informed me, is $12^m.46^s,7$ west of Greenwich, which may also be considered as the longitude of my station. At Leith Fort, on the 17th September, by two sets of altitudes of the sun, taken with the repeating circle and given in the Appendix, the chronometer was found to be $8^m.41^s,6$ too fast, and as it was slow at Greenwich on the 15th June $1^m.15^s.75$, it had lost between that period and the 17th September, $2^m.49^s,35$, which is at the rate of $1^s,8$ daily.

At Unst, by four series of altitudes of the sun, taken on the 22d July with the repeating circle, (which I conceive it is unnecessary to detail, as the results differed very little from each other) the chronometer appeared to be $50^s,2$ fast, to which $1^m.15^s.75$ being added, and also $1^m.6^s,6$ (the loss of the chronometer in 37 days) we obtain $3^m.12^s,55$ for the longitude of Unst in time, west of Greenwich.

Again. Taking Leith for the point of departure, we have the chronometer fast on the 17th September $8^m.41^s,6$, and at

Unst, on the 22d July, $50^{\circ}.2$. The mean of the rates of the chronometer at Unst, Portsoy, and Leith, gives $1^{\circ}.81$ for the mean daily rate, which being multiplied by 57, the number of days between the 22d of July and the 17th September, we have $1^{\circ}.43^{\circ}.17$ for the loss of the chronometer during that period. This being added to $8^{\circ}.41^{\circ}.6$, we obtain $10^{\circ}.24^{\circ}.77$ for the error of the chronometer on the 22d July, for the meridian of Leith, and subtracting $50^{\circ}.2$ (the error at Unst) the remainder $9^{\circ}.34^{\circ}.57$ will be the longitude of Unst, east of Leith. Now the longitude of Leith being $12^{\circ}.46^{\circ}.7$ west, the difference $3^{\circ}.12^{\circ}.13$ will be the longitude of Unst, west of Greenwich. This agreeing so nearly with the preceding result, may perhaps be considered as not very far from the truth.

At Portsoy on the 3d August, the chronometer was found to be $7^{\circ}.52^{\circ}.3$ too fast, by altitudes of the sun, which are detailed in the Appendix. The loss of the chronometer from the 15th June to the 3d August, at the daily rate of $1^{\circ}.8$ is $1^{\circ}.28^{\circ}.2$; which, together with $1^{\circ}.15^{\circ}.75$ (the error of the chronometer at Greenwich on the 15th June) being added to $7^{\circ}.52^{\circ}.3$, we obtain $10^{\circ}.36^{\circ}.25$ for the longitude of Portsoy, west of Greenwich.

In order to deduce the longitude of Portsoy from that of Unst, we have the chronometer fast at Unst on the 22d July $50^{\circ}.2$, and at Portsoy on the 3d August $7^{\circ}.52^{\circ}.3$. The mean of the daily rates at Unst and Portsoy is $1^{\circ}.51$, and the loss from the 22d July to the 3d August, at this rate, is $18^{\circ}.12$. Hence we have Portsoy west of Unst $7^{\circ}.20^{\circ}.22$, and the longitude of Unst from Greenwich being $8^{\circ}.18^{\circ}.87$, we have the

longitude of Portsoy $10^{\circ}.39'.09$ west of Greenwich. The mean of this, and the preceding result being $10^{\circ}.37'.67$, is perhaps not many seconds distant from the truth. I must however remark, that from the variation in the rate of the chronometer, I do not rely upon these longitudes beyond the purpose to which they are to be applied, that of finding the sun's declination at apparent noon.

The instrument used for determining the latitudes, was the repeating circle, of one foot diameter, mentioned at the commencement of this Paper. Of the power of the repeating circle I had ever entertained the most favourable opinion; and I had now an opportunity of bringing it to the test of experiment, by connecting my stations with those of the trigonometrical survey, and comparing the latitudes obtained by the repeating circle with those deduced from observations made with the zenith sector.

As an error in latitude amounting to one minute, would not occasion a difference of one tenth of a vibration of the pendulum in 24 hours, I conceived it would have been an expense of time, which I could ill afford, to have waited for multiplied observations, except at certain stations, the latitudes of which I was anxious to ascertain with particular accuracy.

By the mean of numerous readings, I found the correction for the index error of my instrument to be $+ 18''$; and the value of each division of the large level to be $2''.4$.

In order to deduce the meridional zenith distance, from observations made near the meridian, I availed myself of a very convenient formula, for which I was indebted to Dr. YOUNG, and which has since been published, together with a small table of verse sines, by order of the Commissioners of

Longitude. The refractions and corrections for the barometer and thermometer, are taken from Dr. BRINKLEY's Tables, published with the observations made at the Royal Observatory at Greenwich.

In observations of the sun, the horary angle is estimated in solar time, but in those of the stars it must be expressed in sidereal time. It is most convenient, however, to employ the angle given by the chronometer in finding the correction of the apparent zenith distance, and afterwards to apply a further correction in the following manner.

Let r , be the daily loss of the chronometer on solar or sidereal time, according as the sun or star is observed; and let $r' = \frac{r}{86400 - r}$. Then calling the correction before found C , the final correction will be $(C + 2r'C)$. If the clock gain upon the star, C must be diminished by the quantity $2r'C$.

In using the repeating circle, it is of great importance that its plane should be truly vertical, or that its deviation should be known, in order to find the correction to be added on this account to the observed zenith distance. On my return to London, I found the error of my circle in this respect to be $4'.48''$, the correction for which may be obtained by the following formula:

$$\text{Sin. } \frac{1}{2} (z - z') = \frac{\text{sin. } \frac{1}{2} I.}{\text{tang. } \kappa'}$$

where z is the true zenith distance, z' the observed zenith distance, and I , the angle of inclination of the plane of the circle. In the second member of the equation, z may be taken $= z'$ without error. These formulæ, as well as many others respecting the repeating circle, is demonstrated by M. BRÖR, in his valuable "*Traité élémentaire d'Astronomie Physique.*"

388 Capt. KATER's experiments for determining the variation

At Unst, the following series of observations for the zenith distance of the sun's upper limb was made under the most favourable circumstances. The calculation of the latitude of this, as well as of the other stations, is given at length, to afford the opportunity of any examination that may be thought desirable.

UNST. 23d July, 1818. Barometer 30 inches, thermometer 61°. Time of apparent noon 0^h.6^m.2^s.3. The chronometer too fast 47^s.9. Time by the chronometer at apparent noon 0^h.6^m.50^s.2.

Chronometer.	Level.			Time from Noon.	N. v. Sines.	Readings, &c. O's U. L.	
h. m. s.				m. s.			
23.49.39	+	4	— 5	17.11	2809	First Vernier	123.58.30.00
23.51.46	+	5	— 2	15. 4	2160	Second	58. 0.00
23.54.11	+	3	— 3	12.39	1523	Third	57.55.00
23.56.36	+	2	— 3	10.14	0997	Fourth	58.10.00
23.59.11	+	4	— 1	7.39	0557		
0. 1. 2	+	4	— 0	5.48	0320	Mean	123.58. 8.70
0. 3.29	+	5	+ 2	3.21	0107		+ 360. 0. 0
0. 6. 5	+	3	— 1	0.45	0005	Level	+ 0.48
0. 9.41	+	3	— 1	2.51	0077	Index	+ 0.18
0.11.23	+	4	+ 3	4.33	0197		
0.13.26	+	4	+	6.36	0415		
0.14.59	+	4	+ 3	8. 9	0632		12)483.59.14.70
Mean	+	45	— 5		0817	Observed Z. D.	40.19.56.22
						Refract.	+ 0.48.43
						Paral.	— 0. 5.67
						Semidiam.	+ 15.46.50
						Correct.	— 1.58.83
						Change of Dec.	+ 0.2.16
						(Z—Z')	+ 0.24
						True Z. D.	40.34.29.05
						Dec.	+ 20.10.57.36
						Lat. of Unst.	60.45.26.41
				Const. log.	5.1627253		
				Log. 817 (+4)	6.9122221		
				Corr. —118",83 log.	2.0749474		

$$\frac{(+45-5)}{2} \times 2.4 = +48.0 \text{ correct. for the level.}$$

Lat. 60.45.26 cosine — 9.6888746
 Dec. 20.10.57 cosine — 9.9724798
 Alt. 49.26.33 cos. co. ar. — 0.1869458
 Log. sine 1 co. ar. — 5.3144251

The spot where the above observations were taken, was that selected by M. BIOT, the distance from which to the clock, measured on the meridian northward, was 182 feet = 1,"79.

Adding this to the observed latitude, we have $60^{\circ}.45'.28''.2$ for the latitude of the station where the experiments with the pendulum were made.

The latitude of the spot where M. BIOT's apparatus was fixed, and which was on the same parallel with mine, was determined by Lieut. Col. MUDGE, by connecting it with his station on the island of Balta, where the zenith sector was erected, to be $60^{\circ}.45'.29''.6$. But this latitude is dependent on that of Greenwich, which was taken at $51^{\circ}.28'.40''$. By the observations however of the present Astronomer Royal, and the use of the French refractions, which are very nearly the same as those of Dr. BRINKLEY, the latitude of Greenwich appears to be $51^{\circ}.28'.38''.01$, or 1",99 less than by former observations. This quantity being subtracted from Col. MUDGE's determination, we have $60^{\circ}.45'.27''.61$ for the latitude of the pendulum at Unst, deduced from the Trigonometrical Survey, and $60^{\circ}.45'.28''.2$ by one series of zenith distances of the sun, taken with the repeating circle.

Latitude of Portsoy.

The following series of zenith distances of the sun's upper limb, was taken at the bottom of Mr. WATSON's garden.

390 Capt. KATER's experiments for determining the variation

PORTSOY, 3d Aug. 1818. Barometer 30 inches, thermometer 65°. Time of apparent noon 0^h.5^m.51^s. The chronometer too fast 7^m.52^s.58. (See Appendix.) Time by the Chronometer at apparent noon 0^h.13^m.43^s.58.

Chronometer.	Level.		Time from Noon.	N. v. Sines.	Readings, &c. ☉'s U. L.	
h. m. s.			m. s.			
0. 5.23	+	3	6	8.21	0064	
0. 7.16	+	4	5	6.28	0398	1st Vernier - - 117.35.15.00
0. 9. 8	+	5	7	4.36	0201	Second - - - 34.35.00
0.10. 7	+	2	3	3.37	0124	Third - - - 34.20.00
0.12.17	+	4	7	1.27	0020	Fourth - - - 34.35.00
0.13.36	-	5	3	0. 8	0000	
0.15.43	-	10	6	1.59	0037	Mean - - - 117.34.41.25
0.17.38	-	6	4	3.54	0145	+ 360. 0. 0.00
0.20.36	+	7	9	6.52	0449	Level - - - + 0. 0. 6.00
0.22.30	-	9	7	8.46	0732	Index - - - + 0. 0 18.00
0.24.58	+	1	3	11.14	1201	
0.26.42	-	7	4	12.58	1600	12) 477.35 5.25
Mean .	- 11	+ 16		464		Observed Z. D. - 39.47 55.44
$\frac{(-11+16)}{2} \times 2.4 = +6 \text{ correction for the level.}$					Refract. - + 0.46.43	
Lat. 57.40.57 cosine	-			9.7280375	Paral. - - 0. 5.61	
Dec. 17.37.50 cosine	-			9.9791062	Semidiam. - + 15.47.77	
Alt. 49.56.53 cos. co. ar.	-			0.1914637	Correct - - 1.15.78	
Log. sin. 1 co. ar.	-			5.3144351	Change of Dec. - 0 1.15	
					(Z-Z') + 0.26	
Const. Log.				5.2130325	True Z. D. 40. 3 7.36	
Log. 464 (+4)				6.6665180	Dec. + 17.37 50.03	
Cor. -75".78 Log.				1.8795505	Lat. of Portsoy. 57.40.57.39	

The distance from the place where the latitude was determined to the pendulum, measured on the meridian, was 129 feet, which is equal to 1",26.

This being added to the observed latitude we obtain 57°.40'.58".65 for the latitude of the pendulum.

In order to deduce my latitude from that of Cowhythe, a station was chosen on a small eminence called Portsoy Hill,

294 feet north of the spot where my observations for latitude were made. At this station the oblique angle between Cowhythe and Knock Hill was observed by four repetitions to be - - - - - $117^{\circ}.56'.50''.44$

The zenith distance of Cowhythe - $88.38.40$

————— of Knock Hill - $83.8.51$

Whence the angle between Cowhythe and Knock Hill, reduced to the horizon, is - - $118^{\circ}.21'.35''.64$

Cor. for the excentricity of the telescope + $1,70$

True horizontal angle - $118.21.37,34$

The station at Cowhythe is marked by a conical mass of masonry, which obliged me to place the instrument at the distance of eight feet from its centre, in the direction of Portsoy Hill.

The oblique angle at this spot between Knock Hill and Portsoy Hill, was - - - $54^{\circ}.29'.3''$

The zenith distance of Knock Hill - - $88.30.25$

————— of Portsoy Hill - $91.23.30$

Hence the angle between Knock Hill and Portsoy Hill, reduced to the horizon, is - - - $54^{\circ}.18'.49''$

Reduction to the centre of the station - - $81,5$

Cor. for the excentricity of the telescope - $1,7$

True horizontal angle - $54.18.15,8$

The distance from Cowhythe to Knock Hill, by the trigonometrical survey, is 42633 feet, Knock Hill being to the south west $31^{\circ}.57'.8''$. We have then the following triangle to determine the distance from Cowhythe to Portsoy Hill :

Cowhythe	54.18.15.8	} to Portsoy Hill	{ 6182 —
Knock Hill	7.20. 6.9		
Portsoy Hill	118.21.37.3		

If the angle at Cowhythe be added to $31^{\circ}.57'.8''$, we have $86^{\circ}.15'.23''.8$ for the bearing of Portsoy Hill, to the south-west from Cowhythe, from which and the distance of Cowhythe from Portsoy Hill, we obtain 404 feet for the distance of Portsoy Hill to the south on the meridian.

The latitude of Cowhythe, by the Trigonometrical Survey, is $57^{\circ}.41'.11''$ from which deducting $4''.02$ for the distance on the meridian, $1''.99$ the error of the former latitude of Greenwich, and $2''.92$ the arc due to 294 feet, we obtain $57^{\circ}.41'.2''.07$ for the latitude of my station, deduced from that of Cowhythe, and differing $4''.68$ in excess from the latitude given by the Repeating Circle.

These observations for connecting my station with Cowhythe were made under various unfavourable circumstances, and indeed I am not quite sure that the object I took on Knock Hill was in fact the station; for a pole originally placed in the centre of a cone of masonry, as at Cowhythe, has been taken away, and it was some time before I could decide which to choose among two or three eminences resembling each other, which happen to be upon the hill. The preceding result therefore can be considered only as a proof that no error of consequence is to be feared in my determination of the latitude of Portsoy.

Latitude of Leith Fort.

At Leith Fort, the two following series of observations were made, the sun being frequently obscured by flying clouds.

The first station was at the Flag staff, the second station 48 feet to the south of it.

LEITH FORT, 13th September 1818. Barometer 30.25 inches, thermometer 62°. Time of apparent noon 23^h.55^m57^s.2. The chronometer too fast 8^m.49^s.58. (See Appendix.) Time by the chronometer at apparent noon 0^h.4^m.46^s.78.

Chronometer.	Level.		Time from Noon.	N. v. secs.	Readings, ☉'s U. L.	
h. m. s.			m. s.			
23.55.25	+	3 — 5	9.22	0835	1st. Vernier	- 310.39.15
23.58.39	+	1 — 7	6. 8	0358	Second	- 38.50
0. 1.47	+	7 — 0	3. 0	0086	Third	- 38.50
0. 4.20	+	5 — 3	0.27	0002	Fourth	- 38.55
0.14.16	+	6 — 7	9.29	0856		
0.16.17	+	7 — 6	11.30	1259	Mean	- 310.38.57.50
Mean	+	29 — 28		566	Level	- 1.23
					Index	- 18.00
$\frac{(+29-28)}{2} \times \frac{1}{4} = -1, \text{ s correct for the level.}$						6)310.39.14.30
Lat. 55.58.41 cosine	-		9.7478082		Observed Z. D.	- 51.46.32.38
Dec. 3.56.28 cosine	-		9.9989718		Refract.	+ 1.12.78
Alt. 37.57.47 cosine co. ar.			0.1032492		Paral.	- 6.88
Log sin, 1 co. ar.	-		5.3144251		Semidiam.	+ 15.56.20
					Correct.	- 1.22.66
					Change of Dec.	+ 0. 1.28
					(Z—Z')	+ 0.16
Const. log.			5.1644574		True Z. D.	- 52. 2.13.26
Log. 566 (+4)			6.7528164		Dec.	+ 3.56.27.74
Cor.—82"66 Log.			1.9172738			
					Lat. of the Flag Staff.	55.58.41.00

LEITH FORT, 17th Sept. 1818 Barometer 30.05 inches, thermometer 66°. Time of apparent noon 23^h.54^m.32^s.8. The Chronometer too fast 8^m.42^s.18 (see Appendix.) Time by the chronometer at apparent noon 0^h.3^m.14^s.98

Chronometer.	Level.		Time from Noon.	N. v. Sines.	Readings, &c. @ N. U. I.	
h. m. s.			m. s.			
23.52.28	+ 14	— 7	10.47	1107	1st Vernier	— 319.56.45
23.54.21	+ 10	— 10	8.54	0754	Second	— 30
0.10. 6	+ 25	— 0	6.51	0447	Third	— 3
0.11.26	+ 10	— 15	8.11	0637	Fourth	— 15
0.13. 6	+ 23	— 0	9.51	0923	Mean	— 319.56.30
0.14.19	+ 7	— 15	11. 4	1166	Level	— 50.41
Mean	+ 89	— 47		839	Index	— 18.00
					6) 319.57.38.40	
$\frac{(+89-47)}{2} \times 2.4 = +50.4 \text{ correct. for the level.}$					Observed Z. D.	— 53.19.36.40
Lat. 55.58.41 cosine					Refract.	+ 1.15.85
Dec. 2.24. 2 cosine					Paral.	— 7.03
Alt. 36.25.20 cosine co. ar.					Semidian	+ 15.57.27
Log. sin. 1 co. ar.					Correct.	— 2. 0.24
					Change of Dec.	— 2.63
					(Z—D)	+ 0.15
Const. Log. 5.1562377					True Z. D.	53.34.39.76
Log. 839 (+4) 6.9237620					Dec.	+ 2.24. 1.64
Corr. —120", 23 Log. 2.0799997					55.58.41.39	
					Deduct. for diff. of Stations, (43 ft.)	— 0.43
					Lat. of the Flag Staff	55.58 40.96

By the Trigonometrical Survey, the latitude of the Flag staff of Leith Fort, is 55°.58'.41", but from this 1",99 must be subtracted as before. We have then 55°.58'.39",01 for the latitude of the Flag staff, from which that obtained by the repeating circle under unfavourable circumstances differs 1",97 in excess.

The distance of the clock from the Flag staff was 180 feet to the north, and the corresponding arc 1",8 being added

to $55^{\circ}.58'.39''$, we have $55^{\circ}.58'.40''.8$ for the latitude of the pendulum.

Latitude of Clifton.

In "an account of the measurement of an arc of the meridian," by Lieut. Col. MUDGE, a singular anomaly presents itself, which since the year 1802, when this measurement was made, has been considered with much interest, and in various points of view by the scientific world. Instead of the degrees of the meridian *increasing* with the latitude, as is the case in an oblate spheroid, they appear by this measurement to *decrease*. This remarkable circumstance was examined by Don JOSEPH RODRIGUEZ, in an ingenious paper published in the Philosophical Transactions for 1812. The author proceeding according to a method of verification given by M. DELAMBRE in the "Base Métrique," calculates upon the elliptic hypothesis the length of the whole arc and of each of its parts in seconds, and from the observed latitude of Clifton, the northern extremity of the arc, deduces that of Dunnose, the southern extremity, and of Arbury Hill, an intermediate station which divides the total arc into two nearly equal parts. Don JOSEPH RODRIGUEZ then compares the celestial arcs given by Col. MUDGE's observations, with those resulting from his own calculations, and concludes that the total *observed* arc between Clifton and Dunnose is in excess $1''.38$; that, between Clifton and Arbury $4''.77$; and that the southern portion of the arc between Arbury Hill and Dunnose, is $3''.39$ in defect. The author adds, that "it seems almost beyond a doubt, that it is to errors in the observations of latitude, that the appearance of progressive augmentation of degrees towards the equator is to be ascribed," and that "it is espe-

“cially at the intermediate station at Arbury Hill, that the
 “observations of the stars are erroneous nearly $5''$, notwith-
 “standing the goodness of the instruments and the skill and
 “care of the observer.”

An error at Arbury Hill amounting to $5''$, could scarcely be supposed possible with such an instrument as the zenith sector, in the hands of Col. MUDGE; and the less so, from its appearing that the latitude of Blenheim, deduced trigonometrically from that of Arbury Hill, differed only a fraction of a second from the latitude obtained by the observations made with RAMSDEN'S quadrant at Blenheim observatory. On the other hand, it is not surprising that so great a deviation of the plumb line from the vertical as $5''$,* which would indicate the existence of a disturbing force very nearly equal to that exerted by the mountain Schehallion, should be received with much caution. It became therefore very desirable to endeavour to throw some light on this interesting question, by additional observations at Clifton, Arbury Hill, and Dunnose, for the latitudes of those important stations, an operation to which I felt confident that my repeating circle would not be found inadequate.

Before I proceed to detail the observations made at Clifton, I must observe, that in the repeating circle, as usually constructed in England, the level turns on the axis, and when clamped, is carried with the circle, which renders an additional operation necessary at each repetition, to bring back the level to its former horizontal position. Imagining that if I could obviate this, it would be a considerable saving of time, I had a

* The weight of the plumb line is drawn towards the north and not to the south, as is stated by Col. MUDGE, who probably meant to express the direction of the inclination from the vertical.

398 *Capt. KATER's experiments for determining the variation*

CLIFTON, 5th October, 1818. Barometer 29.0 inches, thermometer 42°, chronometer too fast 21.8. Pole star on the meridian by the chronometer, 12h. 0m. 54.4.

Chronometer.	Level.		Time from the meridian.	N. v. Sines	Readings, &c.	
h. m. s.			m. s.			
11.39.20	+	32	24	21.34	4424	1st Vernier - - - 58.27.50
11.45.11	+	16	39	15.43	2350	Second - - - - 40
11.51.10	+	26	33	9.44	0902	Third - - - - 10
11.55.13	+	30	29	5.41	0307	Fourth - - - - 35
11.58.10	+	26	34	2.44	0071	
12. 1.50	+	31	29	0.56	0008	Mean - - - - 58.27.33.75
12. 5.28	+	25	36	4.34	0198	- 360. 0. 0
12. 8.17	+	32	29	7.23	0519	Level - - - - 1. 9.60
12.12.55	+	36	24	12. 1	1374	Index . - - + 0.18.00
12.16.10	+	23	37	15.16	2218	
12.18.50	+	24	38	17.56	3060	
12.25.10	+	27	34	24.16	5600	(2) 418.26.42.15
	+	328	386		175	Observed Z. D. - - 34.52.13.51
						Refract. - - - + 40.03
						Correct. - - - - 10.96
						2 r'C. - - - - 0.06
						(Z-Z') - - + 0.30
$\frac{(+328-386)}{2} \times 2.4 = -69.6 \text{ cor. for the level.}$						True Z. D. - - 34.52.42.82
Const. Log.	-			3.7959304		Mean P. D. for 1818 + 1.39.44.15
Log. 1753 (+4)	-			7.2437819		Precession, &c. - - 13.17
Cor. -10".96 Log.	-			1.0397123		Co. Lat. 36.32.13.80
						Lat. of Clifton 53.27.46.20

CLIFTON, 6th October, 1818. Barometer 29.20 inches, thermometer 42°, chronometer too fast 1st. Pole star on the meridian by the chronometer 11^h.56^m.56^s.7.

Chronometer.	Level.	Time from the meridian.	N. v. Sines.	Readings, &c.
h. m. s.		m. s.		
11.33.50	+ 30 — 21	23. 7	5083	1st Vernier - - 197.55.50
11.38.33	+ 24 — 29	18.24	3221	Second - - - 25
11.41.25	+ -4 — 53	15.32	2296	Third - - - 20
11.44.25	+ 43 — 12	12.32	1495	Fourth - - - 50
11.46.53	+ 17 — 39	10. 4	0964	
11.50 15	+ 41 — 15	6.42	0427	Mean - - - 197.55.36.25
11.53. 5	+ 24 — 30	3.52	0142	- - - + 360. 0. 0
11.56.25	+ 37 — 19	0.32	0003	Level - - - + 52.80
11.59.23	+ 30 — 25	2.26	0056	Index - - - + 18.00
12. 1.55	+ 32 — 25	4.58	0235	
12. 5.25	+ 36 — 18	8.28	0682	16) 557.56.47.05
12. 8. 5	+ 27 — 30	11. 8	1180	
12.11.15	+ 14 — 44	14.18	1946	Observed Z. D. - - 34.52.17.94
12.14.43	+ 41 — 15	17.46	3003	Refract. - - - + 40.31
12.17.25	+ 26 — 30	20.28	3985	Correct. - - - — 11.80
12.21. 0	+ 40 — 17	24. 3	5501	2 r/C. - - - — 0.06
				(Z—Z') - - - + 0.30
Mean -	+466 — 422		1888	
$\frac{(+466-422)}{2} \times 2.4 = +52.8 \text{ cor. for the level.}$				True Z. D. - - 34.52.46.69
Const. Log.	-	-	3.7959304	Mean P. D. for 1818 + 1.39.44.15
Log. 1888 (+4)	-	-	7.2760020	Precession, &c. - - 13.56
Correct. — 11 ^h .80 Log.	-	-	1.0719324	Co. Lat. 36.32.17.28
				Lat. of Clifton - 53.27.42.72

On comparing the three preceding results, a difference may be perceived between them amounting to 5^h.24; and as I felt assured that the principle of the repeating circle was too perfect to allow of an error of this magnitude, a little reflection led me to discover the cause, to be my fancied improvement in fixing the level to the pillar of the instrument. For in turning the telescope on its axis, the friction, however slight it may be, tends to disturb the relative position of the circle

and level, and thus to introduce error. In the usual construction the level may be clamped to the circle, and then it moves with it without any risk of derangement. This construction was indispensable, in order that the instrument might be used for taking terrestrial angles, and it is to this, perhaps originally accidental circumstance, that the repeating circle is indebted for its very near approach to perfection. After I had restored the instrument to its former state, the following observations were made.

CLIFTON, 8th October, 1818. Barometer 29.60 inches, thermometer 46°. Chronometer too slow 2.9. Pole star on the meridian by the chronometer 11.549^m. 11.

Chronometer.	Level.	Time from the meridian.	N. v. Sines	Readings, &c.
h. m. s.		m. s.		
11.22.10	+ 23 — 24	26.51	6855	1st Vernier - - 178.14.0
11.25.35	+ 26 — 23	23.26	5223	Second - - - 12.30
11.30.20	+ 24 — 24	18.41	3321	Third - - - 12.25
11.33.12	+ 24 — 24	15.49	2380	Fourth - - - 12.35
11.37.10	+ 21 — 27	11.51	1336	
11.40.12	+ 24 — 25	8.49	0740	Mean - - - 178.12.37.1
11.44.7	+ 22 — 28	4.54	0229	+ 360. 0. 0
11.48.10	+ 24 — 35	0.51	0007	Level - - - 8.4
11.53.30	+ 24 — 25	4.29	0191	Index - - + 18.0
11.57.0	+ 26 — 24	7.59	0607	
12. 0.10	+ 29 — 19	11.9	1183	14) 488.12.47.1
12. 3.8	+ 22 — 28	14.7	1896	
12. 6.37	+ 27 — 22	17.36	2947	Observed Z. D. - - 34 52.20.51
12. 9.23	+ 23 — 28	20.22	3946	Refract. - - + 40.52
				Correct. - - - 13.78
	+ 339 — 346		2204	2 ^d C. - - - 0.08
				(Z—Z') - - + 0.30
$\frac{(+339-346)}{2} \times 2.4 = -8.4 \text{ cor. for the level.}$				True Z. D. - - 34 52.47.47
Const. Log. - - -			3.7959304	Mean P. D. for 1818 + 1.39.44.15
Log. 2204 (+4) - - -			7.3432116	Precession, &c. - - - 14.34
Correct.—13 ^{''} .78 Log. - -			1.1391420	Co. Lat. - - 36.32.17.28
				Lat. of Clifton 53.27.42.72

CLIFTON, 12th October, 1818. Barometer 29.56 inches, thermometer 47°, chronometer too slow 9^s. 0. Pole star on the meridian by the chronometer 11^h.33^m.11.6.

Chronometer.	Level.	Time from the meridian.	N. v. Sines.	Readings, &c.
h. m. s.		m. s.		
11.12.40	+ 22 — 26	20.32	4011	1st. Vernier 128.12. 0
11.16.55	+ 26 — 22	16.17	2523	Second - 11.30
11.21.10	+ 24 — 24	12. 2	1378	Third - 11.35
11.24.25	+ 25 — 24	8.47	0734	Fourth - 11.25
11.27.45	+ 25 — 23	5.27	0283	
11.31. 0	+ 25 — 25	2.12	0046	Mean - - 128.11.37.5
11.34.15	+ 28 — 21	1. 3	0010	- + 360. 0. 0
11.37.25	+ 25 — 24	4.13	0169	Level - - + 22.8
11.41.12	+ 24 — 24	8. 0	0609	Index - - + 18.0
11.43.15	+ 25 — 24	10. 3	0961	
11.47.47	+ 25 — 22	13.35	1756	14) 488.12.18.3
11.50.38	+ 25 — 25	17.26	2892	
11.54. 5	+ 25 — 23	20.53	4149	Observed Z. D. - 34.52.18.45
11.56.16	+ 26 — 24	23. 4	5061	Refract. - - + 40.38
				Correct. - - — 10.97
				2 rC. — 0.06
				(Z—Z') - + 0.30
	+ 350 — 331		1755	

$(+350-331) \times \frac{1}{2} = +22.8$ cor. for the level.

Const. Log. - 3.7959304

Log. 1755 (+4) - 7.2442771

Correct.—10^h.97 Log. 1.0402075

True Z. D. - 34.52.48.10
 Mean P. D. for 1818 + 1.39.44.15
 Precession, &c. - — 15.92

Co. Lat. 36.32.16.33

Lat. of Clifton 53.27.43.67

The preceding results in one view are as follow :

53.27.40.94

53.27.46.20

53.27.42.72

53.27.42.72

53.27.43.67

Mean 53.27.43.25

The difference between the two last results which were obtained after the instrument was restored to its original state, is not one second, and the mean of the three first differs only $0''.05$, and of the two last results $0''.06$ from the mean of the whole.

The station where the latitude was observed, was nine feet to the north of the chimney of the room in which the clock was placed; and allowing four feet for the distance of the clock from the chimney, we have $53^{\circ}.27'.43''.12$ for the latitude of the pendulum.

The distance of Laughton Spire from Clifton Beacon, by the Trigonometrical Survey, is 25409 feet, and its bearing $1^{\circ}.56'.12''$ to the south-west. With these data, and the angles observed on the azimuth circle of my instrument, and given in the Appendix, the distance on the meridian, from Clifton Beacon to the chimney of the room where the clock was placed, was found to be 1346 feet, to which nine feet being added, and the arc $13''.36$ corresponding to this distance subtracted from the latitude before found, we have $53^{\circ}.27'.29''.89$ for the latitude of Clifton Beacon.

Before I availed myself of the distance of Laughton spire from Clifton Beacon, I had measured a base of 797 feet for the same purpose, and this gave the distance of the chimney from Clifton Beacon on the meridian 1323 feet; but as I could not see the same part of the chimney from both ends of the base, this determination serves merely to check that before given, and to render it highly probable that there cannot be an error of 10 feet, and perhaps not near so much in the distance first stated.

The observed arc between Greenwich and Clifton Beacon,

by the Trigonometrical Survey, is $1^{\circ}.58'.51''$,59, and this being added to $51^{\circ}.28'.38''$,01 (the latitude of Greenwich) gives $58^{\circ}.27'.29''$,60 for the latitude of Clifton Beacon, differing only $0''$,29 in defect, from the result obtained by the repeating circle, and affording, it is presumed, a satisfactory proof (as far as this instrument is entitled to credit) of the accuracy of the observations made with the zenith sector, both at Clifton Beacon and at Greenwich.

Latitude of Arbury Hill.

The season was so far advanced when I arrived at this important station, that I could not expect numerous observations for the latitude; but from the near agreement of the results at Clifton, I was encouraged to hope that the observations at Arbury Hill, though few in number, might prove satisfactory.

The bell tent was pitched on the *old* station of the Trigonometrical Survey, where the theodolite was placed. This spot may be readily ascertained from Col. MUDGE's description, to within 10 feet. Pickets were driven into the ground, on which rested the legs of a very stout triangular stand, which served as a support to the Repeating Circle. Every precaution which I could think of was used to ensure accuracy. The instrument was adjusted, the telescope directed to the star, and the whole left for nearly half an hour before the commencement of the observations, in order that it might acquire an equal temperature. When the wire was brought very nearly to bisect the star, the tangent screw was turned a little in an opposite direction to release it from any strain, and the hand being withdrawn, the star was watched until its bisection was perfect. The time was then noted, and the level carefully

404 *Capt. KATER's experiments for determining the variation*

read off by the non-commissioned officer and myself, without either of us moving from the place where we stood. In this manner the three following series of observations were made. The error of the chronometer was determined by altitudes of the sun given in the Appendix, and its daily rate was $1^{\circ}.26$.

ARBURY HILL, 18th October, 1818. Barometer 29.40 inches, thermometer $48^{\circ}.5$.

Chronometer too slow $14^{\circ}.7$. Pole star on the meridian by the chronometer

$11^{\text{h}}.9^{\text{m}}.29^{\text{s}}.9$.

Chronometer.	Level.		Time from the meridian.	N. v. Sines.	Readings, &c.	
h. m. s.			m. s.			
10.48 35	+	22	22	20.55	4162	1st. Vernier - - $145^{\circ}.32'.57$
10.53. 5	+	23	22	16.25	2564	Second - - - - - 30
10.57.40	+	21	24	11 53	1344	Third - - - - - 30
11. 1.38	+	24	21	7.52	0589	Fourth - - - - - 35
11. 5. 2	+	24	21	4.28	0190	
11. 8. 0	+	21	24	1.30	0021	Mean - - - - - $145.32.35.5$
11.11.23	+	25	19	1.53	0034	+ 360, 0. 0
11.14.22	+	20	25	4.54	0220	Level - - - - - 3.6
11 18.55	+	24	21	9.25	0844	Index - - - - - + 18.0
11.21.55	+	18	26	12.25	1467	
11.24.55	+	23	21	15.25	2262	14) $505.32.49.90$
11.27.10	+	21	25	17.40	2970	
11.30.47	+	24	21	21.17	4309	Observed Z. D. - - $36. 6.37.85$
11.33.30	+	22	23	24. 0	5478	Refract. - - - + 41.90
						Correct. - - - - - 11.79
	+	312	-315		1890	2'C. - - - - - 0.05
						(Z-Z') - - + 0.28
$(+312-315) \times 2.4 = -3.6$ cor. for the level.					True Z. D. - + $36. 7. 8.19$	
Lat. $52^{\circ}.13.26$ cosine -	9.7871611				Mean P. D. for 1818 + $1.39.44.15$	
Dec. $88.20.30$ cosine -	8.4614886				Precession, &c. - - 18.23	
Alt. $53.52.50$ cosine co. ar.	0.2295379				Co. Lat. - $37.46.34.11$	
Log. sin. 1 co. ar. - -	5.3168000				Lat. of Arbury Hill $52.13.25.89$	
Const. Log. -	3.7949876					
Log. 1890 (+4) -	7.2764618					
Correct. $-11^{\circ}.79$ Log. -	1.0714494					

ARBUCKLE HILL, 22d October, 1818. Barometer 29.40 inches, thermometer 45°
Chronometer too slow 19.7. Pole star on the meridian by the chronometer
 $10^{\text{h}}.53^{\text{m}}.40^{\text{s}}.7$.

Chronometer.	Level.		Time from the meridian.	N. v. Sines.	Readings, &c.	
h. m. s.			m. s.			
10.37. 5	+	24	16.36	2622	1st Vernier	- - 361. 6.30
10.40.15	+	23	13.26	1717	Second	- - 6.25
10.44.45	+	25	8.56	0760	Third	- - 6. 0
10.49.27	+	24	4.14	0171	Fourth	- - 5.55
10.53.25	+	28	0.16	0001		
10.57.45	+	26	4. 4	0157	Mean	- - 361. 6.12.5
11. 1.18	+	28	7.37	0552	Level	- - + 33.6
11.14.45	+	24	21. 4	4222	Index	- - + 18.0
11.17.25	+	27	23.44	5357		
11.19.30	+	25	25.49	6338		
	+	254		2190		10) 361. 7. 4.10
$\frac{(+254-226)}{2} \times 2.4 = +33.6 \text{ cor. for the level.}$					Observed Z. D.	- - 36. 6.42.04
Const. Log. - - 3.7949876					Refract.	- - + 42.23
Log. 2190(+4) - - 7.3404441					Correct.	- - - 13.66
Correct. -13",66 Log. 1.1354317					2 r'C.	- - 0.08
					(Z-Z')	- + 0.28
					True Z.D.	- - 36. 7.10.81
					Mean P.D. for 1818 +	1.39.44.15
					Precession, &c.	- - 19.72
					Co. Lat.	- 37.46.35.24
					Latitude of Arbury Hill	52.13.24.76

The night very clear, but flying clouds.

406 Capt. KATER's experiments for determining the variation

ARBURY HILL, 26th October, 1818. Barometer 29.52 inches, thermometer 47°.5. Chronometer too slow 24°.74. Pole star on the meridian by the chronometer 10^h.37^m.52^s.02

Chronometer.	Level.	Time from the meridian.	N. v. Sines.	Readings, &c.
h. m. s.		m. s.		
10.15.15	+ 22 — 22	22.37	4865	1st Vernier - - 145.33.38
10.18.40	+ 20 — 24	19.12	3507	Second - - 33.3
10.23.0	+ 23 — 21	14.52	2103	Third - - 33.15
10.25.43	+ 23 — 21	12.9	1405	Fourth - - 33.18
10.30.27	+ 21 — 23	7.25	0524	
10.34.5	+ 22 — 23	3.47	0136	Mean - - 145.33.18.50
10.37.20	+ 22 — 22	0.32	0003	Level - - +360.0.0
10.39.50	+ 22 — 22	1.58	0037	Index - - + 3.60
10.43.15	+ 24 — 20	5.23	0276	
10.47.5	+ 20 — 24	9.13	0809	14) 505.33.32.90
10.50.52	+ 24 — 20	13.0	1608	
10.53.25	+ 20 — 24	15.33	2301	Observed Z. D. - 36.640.92
10.57.32	+ 24 — 20	19.40	3680	Refract. - - + 42.15
11.0.32	+ 20 — 24	22.40	4887	Correct. - - 11.64
	+ 307 — 310		1867	2 r°C. - - 0.06
				(Z—Z') - - + 0.28
$\frac{(+307-310)}{3} \times 2.4 = -3.6 \text{ cor. for the level.}$				True Z. D. - 36.7.11.65
Const. Log.	-	3.7949876		Mean P. D. for 1818 + 1.39.44.15
Log. 1867 (+4)	-	7.2711443		Precession, &c. - 21.30
Correct. -11".64 Log.	-	1.0661319		Co. Lat. 37.46.34.50
				Latitude of Arbury Hill 52.13.25.50

The mean of the three preceding results is 52°.13'.25".38, and the greatest difference 1".13.

In the "Account of the Trigonometrical Survey," Col. MUDGE states, that the zenith sector was put up 34 feet to the north, and 28 feet to the west of the old station at Arbury Hill; therefore 0.34" must be added, on this account, to obtain 52°.13'.25".72, the latitude of the spot where the zenith sector was placed.

The observed arc between Greenwich and Arbury Hill, is $0^{\circ}.44'.48''.19$, which being added to the latitude of Greenwich, gives $52^{\circ}.13'.26''.20$ for the latitude of Arbury Hill by the Trigonometrical Survey, which differs $0''.48$ *in excess*, from the latitude given by the Repeating Circle.

We cannot then but conclude, that the observations made with the zenith sector, both at Clifton and Arbury Hill, are free from any material error; and as the difference between the latitudes of Clifton by the Zenith Sector, and by the Repeating Circle, was $0''.29$, that by the Zenith Sector being *in defect*, and of Arbury Hill $0''.48$ *in excess*, it is extremely probable that the error of observation at either of these stations does not amount to so much as four-tenths of a second.

A base of 906 feet was carefully measured near the foot of Arbury Hill, for the purpose of finding the distance on the meridian of this station from the pendulum; which distance, as appears in the Appendix, was 3048 feet, the pendulum being so nearly in the meridian of the station, that no deduction on account of its bearing is necessary. The arc corresponding to 3048 feet, is $30''.06$, which being subtracted from $52^{\circ}.13'.25'.32$, leaves $52^{\circ}.12'.55''.32$ for the latitude of the pendulum.

Latitude of the Station at London.

The latitude of Mr. BROWNE's house in Portland Place, deduced from the Trigonometrical Survey, as detailed in the Philosophical Transactions for 1818, is $51^{\circ}.31'.8''.4$.

Latitude of Shanklin Farm.

Having observed for the latitude of Arbury Hill, at the station itself, it was my intention to have done the same at Dunnose, but this, from the distance of the station, and the difficulty of the ascent, I found impracticable. My observations therefore were made on a spot which was 20 feet south of the chimney of the summer-house in which the pendulum was placed. Previously to quitting London, the transverse level of the repeating circle was adjusted so as to render any correction unnecessary, and the axis carrying the telescope having been tightened, the index error was again carefully determined, and found to be $18''$. The observations were made under circumstances peculiarly favourable, and though those forming the second series are few in number, in consequence of the pole star having been frequently obscured by light clouds, I consider them as unexceptionable. The correction of the mean polar distance for precession, &c. was kindly supplied by the Astronomer Royal.

By altitudes of the sun, given in the Appendix, the chronometer was fast on the 10th of May $4^{\text{h}}.39^{\text{m}}.7$, its daily rate being $-1''.78$.

SHANKLIN FARM, May 13th, 1819. Barometer 30.14 inches, thermometer 47°.0.
Chronometer too fast 4^m.45^s. Pole star on the northern meridian by the chrono-
meter 9^h.37^m.32^s. Mean polar distance for 1819, 1°.39'.24".70.

Chronometer.	Level.		Time from from the meridian.	N. v. Sines.	Readings, &c.	
h. m. s.			m. s.			
9.13.15	+	31	10	24.17	5608	1st Vernier - - 214.14.32
9.16.40	+	8	34	20.52	4142	Second - - - 20
9.21.21	+	21	22	16.11	2492	Third - - - 10
9.33.55	+	17	24	3.37	0124	Fourth - - - 40
9.36.44	+	21	21	0.48	0006	
9.39.55	+	21	21	2.23	0054	Mean - - - 214.14.25.5
9.42.27	+	22	19	4.55	0230	+ 360. 0. 0
9.45.40	+	23	20	8.08	0630	Level - - - 0
9.48.56	+	20	22	11.24	1237	Index - - + 13.0
9.51.53	+	25	18	14.21	1900	
9.54.40	+	20	21	17.08	2793	4) 574.14.38.5
9.56.48	+	19	22	19.16	3531	
9.59.40	+	21	21	22.08	4660	Observed Z. D. - - 41. 1. 2.75
10. 2.20	+	24	18	24.48	5849	Refract. - - + 51.35
						Correct. - - + 13.80
						2 r'C. - - + 0.08
	+	293	+	293	2379	
Lat. 50.37.24 cosine - - 9.8023740					True Z. D. - - 41. 2. 7.98	
Dec. 88.20.29 cosine - - 8.4615613					App. P. D. - - 1.39.31.13	
Alt. 48.57.52 cosine co. ar. - - 0.1827472					Co. Lat. 39.22.36.85	
Log. sin. 1 co. ar. - - 5.3168000					Latitude of Shanklin Farm 50.37.23.15	
Const Log. - - 3.7634825						
Log. 2379 (+4) - - 7.3763944						
Correct. + 13".80 Log. 1.1398769						

110 *Capt. KATER's experiments for determining the variation*

SHANKLIN FARM, May 14th, 1819. Barometer 30.08 inches, thermometer 50° 0.

Chronometer too fast 4^m.43^s. Pole star on the northern meridian by the chronometer 9^h.33^m.35^s.

Chronometer.	Level.		Time from the meridian.	N. v. Sines.	Readings, &c.	
h. m. s.			h. m.			
9.10.30	+ 20	— 16	23.05	5068	1st Vernier	- 328. 8.15
9.13.18	+ 19	— 18	20.17	3914	Second	- 8. 2
9.18. 0	+ 11	— 26	15.35	2311	Third	- 8. 2
9.26.45	+ 17	— 19	6.50	0444	Fourth	- 7.55
9.44. 6	+ 24	— 10	10.31	1053	Mean	- 328. 8. 3.5
9.46.10	+ 13	— 22	12.35	1507	Level	- + 3.6
9.48.15	+ 25	— 10	14.40	2047	Index	- + 13.0
9.51. 6	+ 15	— 20	17.31	2919		
	+ 144	— 141		2408		8) 328. 8.20.1
$\frac{+144-141}{2} \times 2.4 = +3.6$ cor for the level.					Observed Z. D.	- 41. 1. 2.51
Const. Log.	-	-	3.7634825		Refract.	- + 50.92
Log. 2408 (+4)	-	-	7.3816565		Correct.	- + 13.97
Correct. + 13 ^m .97	Log	-	1.1451390		2 P.C.	- + 0.08
					True Z. D.	- 41. 2. 7.48
					App. P. D.	- 1.39 31.32
					Co. Lat.	- 39.22 36.16
					Latitude of Shanklin Farm	50.37.23.84

SHANKLIN FARM, May 15th, 1819. Barometer 30.02 inches, thermometer 43°.5.
Chronometer too fast 4^m.41^s. Pole star on the northern meridian by the chronometer 9^h.29^m.38^s.

Chronometer.	Level.		Time from the meridian.	N. v. Sines.	Readings, &c.	
<i>h. m. s.</i>			<i>m. s.</i>			
9. 6. 3	+	24	— 28	23.35	5215	1st. Vernier - - 296.17.43
9.10. 3	+	24	— 28	19.35	3648	Second - - - 30
9.13.50	+	22	— 30	15.48	2375	Third - - - 40
9.16.52	+	26	— 27	12.46	1551	Fourth - - - 35
9.20.33	+	27	— 23	9. 5	0785	
9.24.35	+	24	— 27	5. 3	0243	Mean - - - 296.17.37
9.27.54	+	25	— 25	1.44	0029	+ 360. 0. 0
9.30.25	+	23	— 27	0.47	0006	Level - - - 44.40
9.33.18	+	24	— 26	3.40	0128	Index - - - 13.0
9.36.17	+	23	— 27	6.39	0421	
9.39. 5	+	25	— 25	9.27	0850	16)656.17. 5.60
9.42.16	+	24	— 26	12.38	1519	
9.44.32	+	23	— 27	14.54	2113	Observed Z. D. - - 41. 1. 4.10
9.47.30	+	25	— 25	17.52	3037	Refract. - - + 51.54
9.50.33	+	21	— 28	20.55	4162	Correct. - - + 11.50
9.54. 0	+	26	— 24	24.22	5647	2 r/C. - - + 0.06
	+	386	— 423		1983	True Z. D. - - 41. 2. 7.20
						App. P. D. - - 1.39.31.51
$(+386-423) \times \frac{1}{2} = -44.4$ cor. for the level					Co. Lat. - - 39.22.35.69	
Const. Log. - - -				3.7634825	Latitude of Shanklin Farm 50.37.24.31	
Log. 1983 (+4) - - -				7.2973227		
Correct. + 11".50 Log. - - -				1.0608052.		

The mean of the three preceding results is 50°.37'.23".77, and the greatest difference 1".16. If to this mean 0".17 be added we have 50°.37'.23".94 for the latitude of the pendulum.

I had now to connect my station with that of Dunnose; a work attended with some difficulty, as Shanklin farm could not be seen from it, and the nature of the ground was very

unfavourable to the measurement of a base. The signal post however was visible from the farm, and I selected the most level part of the hill I could find, on which, with the assistance of Mr. FRANKS, I measured a line of 1140 feet. The angles were taken with the greatest care, and are given with the other necessary data in the Appendix, from which the distance from Dunnose to the chimney of the summer house appears to be 3901 feet, and its bearing $60^{\circ} 58' 11''$ to the north east; whence the distance on the meridian is 1893 feet, or $18^{\circ} 67'$. The distance from the signal post was also calculated, and found to differ only one foot from that of the station.

Fearing from the nature of the ground on which the base was measured, that this determination might be erroneous, I was anxious to verify it by some other method. For this purpose I chose a spot on the side of the hill, which was very level, on which I measured with great care a distance of 100 yards. The direction of this base was perpendicular to a line joining the summer house and the signal post, in which line was also its commencement. I then measured the distance from the signal post to the commencement of the base. By means of eight repetitions with the Repeating Circle, the angle subtended by this base, at a spot 22 feet from the chimney of the summer house towards the signal post, was determined with great precision; and having also the angle of elevation, the horizontal distance from the commencement of the base was obtained, to which 22 feet being added, and also the measured distance from the base to the Signal Post, the result was 3896 feet, for the distance from the Signal Post, to the chimney of the summer house, differing only four feet from the former determination.

If from $50^{\circ}.37'.23'',94$ (the latitude of the summer house) $18'',67$ be subtracted, we have $50^{\circ}.37'.5'',27$ for the latitude of Dunnose by the Repeating Circle.

The latitude of Dunnose is stated in the "Account of the Trigonometrical Survey," to be $50^{\circ}.37'.8'',6$, on the supposition of that of Greenwich being $51^{\circ}.28'.40''$. But this latitude, as before stated, is found from the more recent observations of the present Astronomer Royal, to be $1'',99$ in excess, if the French refractions be employed; therefore $50^{\circ}.37'.6'',61$ is the latitude of Dunnose by the Trigonometrical Survey, differing $1'',34$ in excess from the result obtained by the Repeating Circle.

I may here remark, that the latitude of Dunnose deduced from the observations made with the Repeating Circle, differs only $0'',05$ from the latitude of that station given in the first volume of the account of the survey, and which appears to have been derived trigonometrically from the latitude of Greenwich.

Results of the preceding Operations.

It now remains to give in one view, the results of the operations that have been detailed. These are comprised in the following table. It would have been desirable to have expressed the length of the pendulum vibrating seconds, in parts of the scale which forms the basis of the Trigonometrical Survey of Great Britain, the Commissioners of Weights and Measures having agreed to recommend, that "the standard used in the Trigonometrical Survey of Great Britain should be considered as affording the most authentic determination of the linear measure of the United Kingdom." But as experiments are yet wanting to enable me to do this with sufficient accuracy, I have given the length of the pendulum in parts of Sir GEORGE SHUCKBURN's standard scale, the correction for the difference between which, and the national standard of linear measure, may be readily applied hereafter.

The length of the pendulum vibrating seconds in the latitude of London, is stated in the Phil. Trans. for 1818, to be 39,19860 inches. But I have here to notice a very important omission, which I am obliged to Mr. TROUGHTON for having pointed out in the first number of the Edinburgh Philosophical Journal. It may be seen that in computing the specific gravity of the pendulum, I have neglected to include the deal ends. Anxious to supply this omission in the most unexceptionable manner, I thought it best to take the specific gravity of the whole pendulum, and for this purpose requested Mr. BARTON, Comptroller of his Majesty's Mint, to allow me the use of the fine balance lately constructed under his directions, a request with which he

most obligingly complied, and favoured me with his assistance, and with every requisite for making the experiment.

A deal trough was prepared seven feet long, nine inches wide, and the same depth. The pendulum was slung horizontally from the scale pan, by a fine iron wire. The weight of the whole was carefully determined in air, and found to be 66904 grains. The trough which had been previously placed beneath the pendulum, was then filled with distilled water, and the weight of water displaced was found to be 9066 grains. The small portion of iron wire which was immersed in the water was carefully noted; the weight of the wire by which the pendulum was suspended was 56 grains, and the weight of water equal in bulk to that part of the wire which was immersed was 2,5 grains. The temperature of the water was 68° , and that of the atmosphere 62° ; the barometer 29,9 inches. Hence we have the weight of the pendulum 66858,8 grains in vacuo, at the temperature of 62° ; the weight of an equal bulk of water at the same temperature, 9068,4 grains; and the resulting specific gravity of the pendulum, 7,3727.

Employing this specific gravity in computing the allowance for the mean buoyancy of the atmosphere, we obtain ,00624, for this correction instead of ,00545, the former erroneous conclusion. Besides this, the allowance + ,00031 for the height of the pendulum above the level of the sea, should, according to Dr. YOUNG'S investigation, have been multiplied by $\frac{66}{100}$, making + ,00021 of an inch. These corrections being applied, we have 39,13929 inches of Sir G. SHUCKBURGH'S standard scale, for the length of the pendulum vibrating seconds in the latitude of London.

Wishing to compare with this, the result which would have been obtained by means of the weights and specific gravities of the different parts of the pendulum, I carefully measured

416 *Capt. KATER's experiments for determining the variation*

the deal ends, and found them to contain 3,956 cubic inches. The weight of the knife edges was 370 grains, and their specific gravity 7,84.

With these data, and taking the specific gravity of deal at 0,49; the specific gravity of the whole pendulum will be found to vary from the more accurate determination above given, a quantity which would have occasioned a difference in the length of the seconds pendulum of only $\frac{1}{50000}$ of an inch.

Place of observation.	Latitude.	Vibrations in a mean solar day.	Length of the Pendulum vibrating seconds in parts of Sir George Shuckburgh's scale.
			Inches.
Unst - -	60°.45'.28".01	86096,90	39,17146
Portsoy - -	57.40,58,65	86086,05	39,16159
Leith Fort -	55.58.40,80	86079,40	39,15554
Clifton -	53.27.43,12	86068,90	39,14600
Arbury Hill	52.12.55,32	86065,05	39,14250
London -	51.31. 8,40	86061,52	39,13929
Shanklin Farm	50.37.23,94	86058,07	39,13614

Of the Figure of the Earth.

The deviation of the figure of the earth from a perfect sphere, is expressed by a fraction, having for its numerator the difference between the equatorial and polar diameters, and for its denominator the diameter at the equator; this is termed the *compression* or *ellipticity*.

If the earth were a perfect sphere, composed of homogeneous materials, as a fluid, and at rest, gravity at every point in its surface would be the same. But if this sphere were made to revolve about an axis, its particles would endeavour to fly off with a centrifugal force proportionate to the distance from the axis of rotation; the equatorial parts would become elevated, those at the pole and its vicinity depressed, and the sphere would assume the form of a spheroid, the centrifugal force thus generated acting in opposition to gravity, and diminishing it more and more from the Pole, where the centrifugal force is nothing, to the Equator where it is a maximum.

But besides this diminution of gravity from centrifugal force, in proceeding from the pole to the equator, a farther reduction takes place in consequence of the elliptical form which the earth has now assumed. For the parts about the Pole being nearer to the centre of the spheroid than those at the Equator, will be more strongly attracted, and this farther reduction of gravity, whatever it may be, varies with the figure of the earth, and as we shall presently see, with a variation in the density of the strata of which it is composed.

If we conceive two fluid columns meeting in the centre of such a spheroid, the one proceeding from the Pole and the

other from the Equator, it follows in order that the spheroid may preserve a state of equilibrium, that the pressure of the equatorial and polar columns on the centre must be equal. The equatorial column then has been lengthened in proportion to the diminution of its gravity. The ellipticity therefore, and the diminution of gravity from the Pole to the Equator, will, on this supposition of a homogenous spheroid, be expressed by the same fraction, which NEWTON has demonstrated to be $\frac{1}{230}$.

If now we suppose new matter to be added to the centre of such homogeneous spheroid, or its density there to be increased, this matter, by its additional attraction, will cause a greater increase of gravitation at the Pole than at the Equator, in consequence of the distance from the Pole to the centre being the less; but the equatorial column being the longer, and therefore consisting of a greater quantity of matter, its gravity or pressure on the centre will be more increased by this new attraction than that of the polar column; and in order to restore the equilibrium thus destroyed, the polar column must become longer, and the equatorial column shorter than before. Thus the ellipticity of the spheroid will be diminished, but the difference of gravitation at the Pole and at the Equator will, at the same time, be increased.

HUYGENS considered the whole attractive force to reside in the centre, or the earth to be infinitely dense there, and on this supposition, computing its ellipticity, he found it to be $\frac{1}{178}$.

But experiments with the pendulum soon sufficiently proved that the earth was neither homogeneous, nor, it is scarcely necessary to say, infinitely dense at its centre; but that it

probably increased in density from the surface to the centre, the ellipticity being consequently somewhere between $\frac{1}{578}$ and $\frac{1}{230}$.

As it appears then that the ellipticity of the earth varies with any difference in the diminution of gravitation from the Pole to the Equator, and that this last depends in its turn on the ellipticity; it might have been supposed that any attempt to arrive at the figure of the earth in this way must have been hopeless.

But it was reserved for CLAIRAUT to remove this difficulty. He found that however the density of the earth be supposed to vary, the fraction expressing its ellipticity increases as the fraction expressing the diminution of gravity from the pole to the equator diminishes, and vice versa; and in his admirable work on the figure of the earth, he has demonstrated this beautiful and important theorem; that *the sum of the two fractions expressing the ellipticity and the diminution of gravity from the Pole to the Equator, is always a constant quantity, and equal to $\frac{2}{3}$ of the fraction expressing the ratio of centrifugal force to that of gravity at the equator.*

If then the decrease of gravity from the Pole to the Equator can be discovered, and it be subtracted from this constant quantity, the remainder will be the fraction expressing the ellipticity of the spheroid.

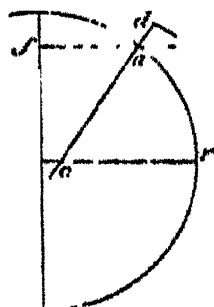
The diminution of gravity may be known by finding the difference of the lengths of the two pendulums vibrating in equal times at the Pole and at the Equator, as it may be easily demonstrated that the lengths of such pendulums are to each other directly as gravitation; or, if an invariable pendulum, such as I have used, be employed, the squares of the observed

number of vibrations in 24 hours, in different latitudes, will be to each other as gravitation in such latitudes.

But as experiments on the pendulum cannot be made at the Pole, it remains to describe the manner in which the diminution of gravity from the Pole to the Equator, may be obtained by observations made at intermediate stations.

I have remarked, that the centrifugal force varies as the distance from the axis of rotation; that is as the cosine of the latitude; thus at the Equator it is the greatest, at the Poles it is nothing.

But the whole of the centrifugal force does not act in opposition to gravity except at the Equator; for let cd be the direction of gravity, cb that of centrifugal force, and let the centrifugal force for the latitude a , be expressed by the line ab ; if this be resolved into two forces ad and db , that portion which acts in opposition to gravity will be expressed by ad . But if ab be made the radius, ad is the cosine of the angle adb , = acr , the latitude of the point a . The effect then of the centrifugal force at a , in counteracting gravity, is still farther diminished in the proportion of the cosine of the latitude to the radius; whence it follows, that the diminution of gravity from this cause, in proceeding from the Pole to the Equator, will be as the difference of the squares of the cosines of the latitudes.



From the expression for the force of gravity at the surface of a spheroid,* we may readily perceive that that part of the

* $f = \frac{4\pi b}{3} \left(1 + \frac{c}{b} \cdot \frac{4 - \sin^2 \phi}{5} \right)$ in which the $\sin^2 \phi$ is the only variable quantity, ϕ being the angle of the terrestrial radius with the Equator.

diminution which depends on the elliptical form of the earth, follows very nearly the same law; therefore the increase of gravitation in proceeding from the Equator to the Pole, may be taken as the increase of the square of the sine of the latitude;* and this will also express the corresponding variation in the length of the pendulum.

Let E = The length of the pendulum vibrating seconds at the Equator.

d = The difference between the length at the Equator and at the Pole.

m = The length of the pendulum in the latitude L .

n = The length of the pendulum in the latitude L' .

Then from what has been stated,

$$m = E + d \cdot \sin^2 L$$

$$n = E + d \cdot \sin^2 L'$$

$$m - n = (E + d \sin^2 L) - (E + d \sin^2 L') = d(\sin^2 L - \sin^2 L')$$

$$\text{Hence } d = \frac{m - n}{\sin(L + L') \times \sin(L - L')}$$

$$\text{and } E = m - (d \sin^2 L.)$$

Therefore $\frac{d}{E}$ expresses the diminution of gravity from the Pole to the Equator, which being subtracted from $\frac{5}{8}$ of the proportion of centrifugal force to gravity at the Equator, will give the ellipticity of the spheroid.

The centrifugal force at the Equator is expressed by the deflection of a point on its surface from the tangent, in one second of mean solar time. This is equal to the versed sine of $15'',0418$, the arc which the earth describes in its diurnal revolution in one second; and taking the radius of the Equator at 3967,5 miles, is found to be ,055696 of a foot.

* The \sin^2 + the cosine 2 is a constant quantity, equal to the radius 2 , consequently as the cosine 2 diminishes, the sine 2 must increase, and vice versa.

If g , be the space a body falls through in one second of time at the Equator, L the length of the seconds pendulum, and c the circumference of a circle, the diameter being 1,

$$g = \frac{1}{2} L \times c^2.$$

The length of the pendulum vibrating seconds at the equator, deduced from the observations at Unst and Dunnose, by the preceding formula, appears to be 39,00734 inches, and g , or gravitation at the Equator, to be equal to 16,0412 feet. Hence the centrifugal force at the equator is $\frac{1}{248,013}$ of gravitation, or $\frac{1}{280,014}$ of gravity; which last being multiplied by $\frac{5}{2}$, we have ,0086501 for the sum of the fractions expressing the ellipticity of the earth and the diminution of gravity, from the Pole to the Equator.

In the following Table are given the diminution of gravity from the Pole to the Equator, and the resulting compression, deduced in the manner which has been described, by comparing the observations at each station, successively with those at all the others.

	Diminution of gravity from the Pole to the Equator.	Compression.
Unst and Portsoy - -	,0053639	$\frac{1}{304,3}$
Leith Fort - -	,0054840	$\frac{1}{315,8}$
Clifton - -	,0056340	$\frac{1}{331,5}$
Arbury Hill -	,0054282	$\frac{1}{310,3}$
London - -	,0055510	$\frac{1}{322,7}$
Dunnose - -	,0055262	$\frac{1}{320,1}$
Portsoy and Leith Fort -	,0056920	$\frac{1}{338,0}$
Clifton - -	,0058194	$\frac{1}{353,2}$
Arbury Hill -	,0054620	$\frac{1}{313,7}$
London - -	,0056382	$\frac{1}{332,0}$
Dunnose - -	,0055920	$\frac{1}{326,9}$
Leith Fort and Clifton -	,0059033	$\frac{1}{364,0}$
Arbury Hill -	,0053615	$\frac{1}{304,1}$
London - -	,0056186	$\frac{1}{329,8}$
Dunnose -	,0055614	$\frac{1}{323,7}$
Clifton and Arbury Hill -	,0042956	$\frac{1}{229,6}$
London -	,0052590	$\frac{1}{294,9}$
Dunnose -	,0052616	$\frac{1}{295,1}$
Arbury Hill and London -	,0069767	$\frac{1}{597,5}$
Dunnose -	,0060212	$\frac{1}{380,3}$
London and Dunnose -	,0052837	$\frac{1}{297,0}$

From the experiments given in the former part of this

Report, it appears probable, that if the uncertainty which must exist in the allowance for the height above the level of the sea be excepted, the error in the number of vibrations of the pendulum at any particular station, does not amount to so much as one tenth of a vibration, which is nearly equivalent to $\frac{1}{4000000}$ part of the length of the seconds pendulum. To this degree of accuracy consequently may gravitation be determined by the apparatus I have employed; and in passing through a country composed of materials of various densities, the pendulum may be expected to indicate such variation with very considerable precision.

The diminution of gravity from the Pole to the Equator is derived from the decrease which is observed to take place between any two given latitudes; consequently if no irregular attraction occurred, the results, computed from different portions of the meridian, should be the same. But it may be seen in the preceding table, that the number expressing the diminution of gravity, from the observations at Unst and Portsoy, is less than that deduced from the arc between Unst and Leith, and that this number goes on increasing to Clifton, diminishes at Arbury Hill, and increases again at London. It may also be remarked, that the diminution of gravity, derived from Unst and Dunnose, is less than that deduced from Portsoy and Dunnose; from all which it seems probable that in advancing southward, gravity decreases more than it ought to do from theory; that there exists an assemblage of materials of greater density than common in the vicinity of Portsoy, and that the density of the strata to the southward becomes less and less until we arrive at Clifton, where it seems to be considerably in defect.

At Arbury Hill, a sudden increase of gravitation is percep-

tible, and at the short distance of London, this additional force is no longer sensible. From its intensity, and the limited sphere of its action, it might perhaps be inferred that the disturbing material is of considerable density, and not very distant from the surface.

It must be evident that nothing very decisive respecting the general ellipticity of the Meridian can be deduced from the present experiments. For this purpose it is requisite that the extreme stations should comprise an arc of sufficient length to render the effect of irregular attraction insensible; and this effect might be diminished, if not wholly prevented, by selecting stations of similar geological character, and which should differ as little as possible in elevation above the level of the sea.

If however some deduction be made for the superior density which it has been remarked exists at Portsoy, the compression $\frac{1}{364}$ deduced from that station and Unst, may perhaps be considered as not far distant from the truth, both being situated on rocks of a similar nature; Unst consisting chiefly of serpentine, and Portsoy, of serpentine, slate, and granite; and as $\frac{1}{316}$ the ellipticity given by the experiments at Unst and Arbury Hill, is nearly the same as that resulting from Unst and Portsoy, it would be no improbable conjecture that the sudden increase of gravitation observed at Arbury Hill, may be occasioned by a rock of primitive formation, approaching the surface of the earth in the vicinity of that station.*

These facts appear sufficient to explain the anomalies which

* Since the above was written, I find the conjecture I have hazarded remarkably supported by fact; for on consulting SMITH'S Geological Map of England, it appears that Mount Sorrel, a mass of granite, is situated, together with other rocks of primitive formation, about 30 miles to the north of Arbury Hill.

have been remarked in the Trigonometrical Survey of Great Britain. For if the disturbing force in the neighbourhood of Arbury Hill, were supposed to be situated to the north of that station, the plumb line would be attracted northward, the observed latitude would be less than the true, and the length of the degree deduced from the arc between Dunnose and Arbury would be in excess, and that derived from the arc between Clifton and Arbury in defect. This last error will be augmented, if we suppose the attraction of the matter near Arbury Hill to be felt at Clifton, and the plumb line at that station to be drawn towards the south.

M. BIOT, by a comparison of his numerous experiments at Unst with those made at Formentara and Dunkirk, in conjunction with M. ARAGO, obtains $\frac{1}{15}$ for the resulting compression. But if the allowance for the elevation of Formentara above the level of the sea, be corrected in the manner suggested by Dr. YOUNG, the ellipticity should be about $\frac{1}{6}$. The details of M. BIOT's experiments have not yet been published, but it affords me much gratification to learn, that the acceleration of the pendulum between London and Unst, computed by M. BIOT, from his observations at Unst and those at Formentara, using $\frac{1}{318}$ for the compression, differs only 0.6 from the result of my experiments; a difference which may probably be referred to the superior density of Unst, compared with that of the substrata of London.

APPENDIX.

CONTAINING THE OBSERVATIONS FROM WHICH THE
PRECEDING RESULTS WERE COMPUTED.

Observations for determining the rate of the clock.

WITH respect to the following Table of Transits it may be necessary to remark that the results in the column headed "Mean Chronometer," were obtained by taking the mean of the 1st and 5th wires, of the 2d and 4th, and again taking the mean of these means and the third wire, instead of taking the mean of the five wires, which is the usual method. This was done for the sake of comparing the result of each pair of wires with that of the meridian wire.

Transits observed at Uxer.

Date.	Stars.	1	2	Merid. wire. 3	4	5	Mean Chronometer.	Clock.
1818. July 22	Arcturus " Ophiuchi " Serpentis " Lyrae " Orionis	h. m. s. 6. 8.14,0 9. — 10.12.15,0 10.30.42,5 21.43.41	m. s. 8.36,5 — 12.36,0 31. 9,5 44. 2,5	m. s. 8.59,0 50. 9,5 12.57,0 31.36,5 44.23,5	m. s. 9.21,5 — 13.18,5 32. 4,0 44.45,0	m. s. 9.44,0 — 13.39,5 32.30,5 45. 6,5	h. m. s. 6. 8.59 9.50. 9,5 10.12.57,17 10.31.36,58 21.44. 23,67	h. m. s. 6.14.18,14 9.55.36,41 10.18.25,08 10.37. 5 21.50.15,3
24	☉'s centre.	0. 6.27	6.49,5	7.12,25	7.35	7.57	0. 7.12,17	0.13.59,32
25	" Ophiuchi " Ophiuchi " Serpentis " Lyrae	9.15. 2,5 9. — 10. 0.23,5 10.18.50,5	15.24 37.57,5 0.45,5 19.17,5	15.45,5 38.19 1. 6,5 19.44	16. 7,5 38.40,5 1.27,5 20.12	16.29 — 1.49 20.38,5	9.15.45,62 9.38.19 10. 1. 6,41 10.19.44,41	9.23.43,56 9.46.18,11 10. 9. 6,1 10.27.44,82
26	☉'s centre.	0. 6.24,5	6.46,75	7. 9,25	7.31,75	7.54,5	0. 7. 9,33	0.15.41,63

Oil was applied to the scapeament without stopping the clock.

Date.	Stars.	1.	2.	Mend. wire. 3.	4.	5.	Mean Chronometer.	Clock.
		h. m. s.	m. s.	m. s.	m. s.	m. s.	h. m. s.	h. m. s.
July. 27	α Orionis	21.23.55.5	—	—	—	—	21.24.18.17	21.14.18.69
28	\odot 's centre	0.6.23.75	6.46.25	7.8.5	7.31	7.53.5	0.7.8.55	0.17.25.41
	Arcturus	5.44.31	44.53	45.15.5	—	—	5.45.1.00	5.55.45.27
	α Ophiuchi	9.3.10	3.32	3.53.5	4.15.5	4.31	9.3.53.56	9.14.30.51
	γ Ophiuchi	9.25.43.5	26.5	26.26.5	26.48.5	27.0.5	9.26.26.55	9.37.4.48
	η Serpentis	9.48.32	48.53.5	49.14	49.35.5	49.57.0	9.49.14.03	9.59.52.73
	α Lyrae	10.6.58.5	7.25.5	7.52.5	8.20	8.47	10.7.52.67	10.18.12.11

Transits observed at Portroy—1st. Series.

Aug. 5.	γ Ophiuchi	9.0.51	1.12.5	1.34	1.56	2.17	9.1.34.06	9.2.15.99
	η Serpentis	9. —	24.2	24.23.5	24.41.5	25.5.5	9.24.23.30	9.25.25.99
	α Lyrae	9.42.19.5	42.46	43.13	43.40.5	44.7.5	9.43.13.27	9.44.16.20
	β	10.14.42.5	15.4.5	15.26	15.48	16.0.5	10.15.29.08	10.16.29.86
	μ Aquilae	10.36.46.5	37.8	37.29	37.51	38.12	10.37.29.25	10.38.33.58
	α Aquilae	10.53.26	—	54.9	54.30.5	54.51.5	10.54.8.88	10.55.13.62
6	\odot 's centre	0.12.35.25	12.57.5	13.19.75	13.41.75	14.4	0.13.19.66	0.14.41.62
	α Ophiuchi	8.34.25.5	34.47.5	35.9.0	35.30.5	35.52	8.35.8.92	8.36.43.60
7	\odot 's centre	0.12.28	12.50.25	13.12.25	13.34.5	14.56.5	0.13.12.29	0.15.7.95
8	\odot 's centre	0.12.19.75	12.41.75	13.3.50	13.26	13.47.5	0.13.3.66	0.15.36.56
	Arcturus	5.7.14	8.16.5	8.39	9.2	9.24.5	5.8.19.17	5.11.20.51
	α Ophiuchi	8.26.31.5	26.53	27.14.5	27.37	27.58	8.27.14.56	8.30.1.45
	γ Ophiuchi	8.48.59.5	49.21	49.43	50.4	50.15	8.49.42.25	8.52.29.70
	η Serpentis	9.11.49	12.10.5	12.31.5	12.53	13.14.5	9.12.31.66	9.15.19.73
	α Lyrae	9.30.27.5	30.54	31.21.5	31.49	32.16	9.31.21.58	9.34.10.19
	α Aquilae	10.41.35	41.56	42.17.5	42.39	43.0.5	10.42.17.58	10.45.8.14
10	\odot 's centre	0.12.22.5	12.23.75	12.45.75	13.8.25	13.29.75	0.12.45.92	0.16.38.83
	Arcturus	5.0.0	0.22.5	0.45	1.7.5	1.30	5.0.45	5.4.46.51
	α Ophiuchi	8.18.38	18.59.5	19.21	19.43	20.4.5	8.19.21.16	8.23.28.67
	γ Ophiuchi	8.41.6	41.37	41.48.5	42.10.5	42.31.5	8.41.48.66	8.45.56.85
	η Serpentis	9.3.55.5	4.16.5	4.37.5	4.59.5	5.20.5	9.4.37.83	9.8.46.76
	α Lyrae	9.22.34	23.1	23.27.5	23.55.5	24.22	9.23.27.92	9.27.37.34
	β	9.48.30	48.51.5	49.12	49.34	49.55	9.49.12.42	9.53.22.62
	δ	9.54.37.5	55.19	55.40.5	56.3	56.24.5	9.55.40.83	9.59.51.24
	α Aquilae	10.33.41	34.2	34.23.5	34.45.5	35.6.5	10.34.23.66	10.38.35.49
11	α Ophiuchi	8.14.39	15.0.5	15.22	15.44	16.5.5	8.15.22.17	8.20.13.59
	γ Ophiuchi	8.37.7	37.28.5	37.49.5	38.11.5	38.32.5	8.37.49.75	8.42.41.94
	η Serpentis	8.59.57	60.18	60.39	61.0.5	61.21.5	9.0.39.17	9.5.32.03
	α Lyrae	9.18.35.5	19.2	19.29	19.56.5	20.23.5	9.19.29.10	9.24.22.52
	β	9.44.31	44.52.5	45.13.5	45.35	45.56	9.45.13.58	9.50.7.82
	δ	9.50.58.5	50.20.5	51.42	52.4	52.25.5	9.51.42.08	9.56.36.52
	μ Aquilae	10.13.3	13.24	13.45	14.7	14.28	10.13.45.33	10.18.40.52
	α Aquilae	10.29.42	30.3.5	30.25	30.46.5	31.7.5	10.30.24.92	10.35.20.47

Date.	Stars.	1.	2.	Merid. wire. 3.	4.	5.	Mean Chronometer.	Clock.
Aug. 12	☉'s centre	h. m. s. 0.11.38,5	m. s. 12. 0	m. s. 12.22	m. s. 12.44,25	m. s. 13. 6	h. m. s. 0.12.22,12	h. m. s. 0.17.42,63
	Arcturus	4.52. 3	52.25,5	52.48	53.10,5	53.33	4.52.48	4.58.16,63
	α Ophiuchi	8.10.40,5	11. 2	11.24	11.45,5	12. 7	8.11.23,83	8.16.58,52
	β Ophiuchi	8.33. 8,5	33.30	33.51,5	34.13	34.34,5	8.33.51,5	8.39.26,94
	γ Serpenteis	8.55.58,5	56.19,5	56.40,5	57. 2	57.23	8.56.40,67	9. 2.17,04
	α Lyrae	9.14.36,5	15. 3,5	15.30,5	15.58	16.24,5	9.15.30,58	9.21. 7,43
	α	9.40.33	40.54	41.15	41.36,5	41.57,5	9.41.15,17	9.46.52,91
	β	9.47. 0,5	47.22	47.43,5	48. 5,5	48.27	9.47.43,67	9.53.21,61
	μ Aquilæ	10. 9. 4	9.25	9.46,5	10. 8,5	10.29,5	10. 9.46,67	10.15.25,11
	α Aquilæ	10.25.43,5	26. 5	26.26	26.48	27. 9	10.26.26,25	10.32. 5,33

Transits observed at Portsoy—2d Series.

Aug. 13	☉'s centre	0.11.17	11.48,75	12.10,5	12.32,5	12.54,5	0.12.10,62	0.11.47,72
	Arcturus	4.48. 5	48.27,5	48.50	49.12,5	49.35	4.48.50	4.48.35,44
	α Ophiuchi	8. 6.42,5	7. 4	7.25,5	7.47,5	8. 9	8. 7.25,67	8. 7.17,18
14	α Ophiuchi	8. 2.45	3. 6,5	3.28	3.50	4.11,5	8. 3.28,17	8. 4. 2,83
	β Ophiuchi	8.25.13,5	25.34,5	25.56	26.17,5	26.39	8.25.56,08	8.26.31,46
	γ Serpenteis	8.48. 3,5	48.24	48.45	49. 6,5	49.27,5	8.48.45,25	8.49.21,19
	α Lyrae	9. 6.41	7. 8	7.35	8. 2,5	8.29,5	9. 7.35,17	9. 8.11,68
	μ Aquilæ	10. 1. 9	1.30	1.51,5	2.13	2.34	10. 1.51,5	10. 2.29,88
	α Aquilæ	10.17.48	18. 9,5	18.31	18.52,5	19.13,5	10.18.30,92	10.19. 9,84
15	☉'s centre	0.11. 0,5	11.22,25	11.44	12. 6,25	12.27,75	0.11.44,12	0.12.49,27
	Arcturus	4.40. 9	40.31	40.53,5	41.16	41.39	4.40.53,5	4.42. 6,66
	α Ophiuchi	7.58.46,5	59. 8	59.29,5	59.51,5	60.13	7.59.29,67	8. 0.48,78
	β Ophiuchi	8.21.14	21.35,5	21.57	22.18,5	22.40	8.21.57	8.23.16,94
	γ Serpenteis	8.44. 4,5	44.25	44.46	45. 7,5	45.29	8.44.46,33	8.46. 7
	α Lyrae	9. 2. 42	3. 9	3.36	4. 3,5	4.30,5	9. 3.36,17	9. 4.57,59
16	☉'s centre	0.10.47,75	11. 9,2	11.31	11.53	12.14,5	0.11.31,08	0.13.10,72
	Arcturus	4.36.11,5	36.33,5	36.56	37.19	37.41	4.36.56,17	4.38.53,17
	α Ophiuchi	7.54.48,5	55.10	55.31,7	55.53,7	56.15	7.55.31,73	7.57.34,88
	γ Serpenteis	8.40. 6,5	—	40.48,7	41.10	—	8.40.48,67	8.42.53,09
	α Lyrae	8.58.44,7	59.11,8	59.38,5	60. 6	60.32,9	8.59.38,73	9. 1.43,81
17	☉'s centre	0.10.33,55	10.55,25	11.16,75	11.38,65	12. 0,45	0.11.16,9	0.13.49,88
	α Ophiuchi	7.50.50,7	51.12,3	51.34	51.56	52.17,3	7.51.34	7.54.21,23
	β Ophiuchi	8.13.19	13.40,1	14. 1,5	14.23,2	14.44,5	8.14. 1,61	8.16.49,55
	γ Serpenteis	8.36. 9	36.39,9	36.51	37.12,3	37.33,3	8.36.51,08	8.39.39,77
	α Lyrae	8.54.46,6	55.13,7	55.40,7	56. 8	56.35	8.55.40,77	8.58.30,19
	μ Aquilæ	9.49.14,7	49.35,8	49.57	50.18,7	50.39,8	9.49.57,17	9.52.48,21
	α Aquilæ	10. 5.54	6.15,3	6.36,6	6.58,2	7.19,3	10. 6.36,67	10. 9.28,18
18	Arcturus	4.28.16,7	28.39,3	—	—	29.46,7	4.29. 1,70	4.32.25,84
	β Ophiuchi	8. —	9.43,7	—	10.26,8	10.48	8.10. 5,18	8.13.36,30
	γ Serpenteis	8.32.12	32.33,3	32.54,3	—	33.36,8	8.32.54,42	8.36.26,36
	α Lyrae	8. —	—	51.44	52.11,3	52.38	8.51.43,95	8.55.16,39

Date.	Stars.	1.	2.	3.	4.	5.	Mean Chronometer.	Clock.
Aug. 19	☉'s { 1st limb 2d limb	h. m. s. 0. 9. 0.7 —	m. s. 9.22.2 11.32.5	m. s. — 11.54	m. s. 10. 6.0 —	m. s. — 12.37.5	h. m. s. 0.10.19.15 —	h. m. s. 0.14.49.28 —

Transits observed at LEITH FORT.—1st Series.

31	♈ Capricorni	9.49.25.1	49.47.5	50. 9.5	50.32	50.54.3	9.50. 9.65	9.49.41.04
	♈ Equulei	10.37.31.7	—	38.14	38.35	38.56.3	10.38.14	10.37.46.18
	♈ Aquarii	10.52.45	53. 6.2	53.27.3	53.49	54.10	10.53.27.47	10.52.59.93
	♈ Pegasi	11. 5.57	6.18.5	6.30.5	7. 1	7.22.7	11. 6.39.7	11. 6.12.53
	♈ Aquarii	11.24.35.1	24.56	25.17	25.38.3	26. 1	11.25.17.25	11.24.50.38
	♈ Aquarii	11.42.53.4	43.14.2	43.35.3	43.56.5	44.18	11.43.35.45	11.43. 8.89
	♈ Aquarii	11. —	59.16.7	59.37.8	59.59	—	11.59.37.85	11.59.11.46
Sept. 2	☉'s { 1st limb 2d limb	— 0. 9.20	7.32.7 9.41.5	7.54 10. 2.5	8.15.8 10.24	8.37 10.45.5	0. 8.58.43	0. 9.18.05
	♈ Capricorni	9.41.24.5	41.46.8	42. 9	42.11.1	42.53.7	9.42. 9.07	9.42.40.21
	♈ Aquarii	10. 0.42.4	1. 3.8	1.25.3	1.47	2. 8.4	10. 1.25.37	10. 1.56.92
	♈ Equulei	10.29.30.9	29.51.9	30.13	—	—	10.30.13.1	10.30.45.31
	♈ Aquarii	10.44.45	45. 5.5	45.27	45.48.3	46. 9.7	10.45.27.07	10.45.59.62
	♈ Aquarii	11.16.34.5	16.55.7	17.16.5	17.18	17.59	11.17.16.7	11.17.49.91
	♈ Aquarii	11.53.57.1	54.18.3	54.39.5	55. 0.8	55.22	11.54.39.53	11.55.13.67
	♈ Pegasi	12. 0. 6.8	0.28.3	0.49.8	1.11.5	1.33	12. 0.19.87	12. 1.24.05
4	♈ Capricorni	9. —	—	34.12	34.34.3	34.56.6	9.34.11.98	9.35.42.16
	♈ Aquarii	9.52.45.5	53. 7	53.28.3	53.50	54.11.4	9.53.28.42	9.54.59.09
	♈ Equulei	10.21.33.6	21.54.8	22.16	22.37	22.58.3	10.22.15.95	10.21.47.39
	♈ Aquarii	10.36.47.3	37. 8.5	37.29.5	37.51	38.12.3	10.37.29.68	10.39. 1.37
	♈ Pegasi	10.49.59.2	50.20.5	50.41.8	51. 3.5	51.25	10.50.41.99	10.52.13.94
	♈ Aquarii	11. 8.37.1	8.58.3	9.19.4	9.40.8	10. 2	11. 9.19.5	11.10.51.95
	♈ Aquarii	11.26.55.6	27.16.5	27.37.5	27.59	28.19.9	11.27.37.67	11.29.10.60
	♈ Aquarii	11. —	43.18.8	43.39.9	44. 1	44.22.3	11.43.39.99	11.45.13.97
	♈ Pegasi	11. —	52.31.7	52.52.5	53.14.5	53.36	11.52.54.82	11.54.26.26
5	☉'s { 1st limb 2d limb	— 0. 8.13.3	— 8.34.9	6.47.8 8.56.2	7. 9.2 9.17.5	7.30.2 9.38.8	0. 7.51.93	0. 9.41.66
6	☉'s { 1st limb 2d limb	0. 5.42.8 0. 7.51	6. 4 8.12	6.25 8.33.3	6.46.4 8.55	7. 8 9.16	0. 7.29.35	0. 9.51.50

Transits observed at LEITH FORT.—2d Series.

8	♈ Equulei	10. 5.41	6. 2.4	6.23.4	6.45	7. 6	10. 6.23.53	10. 2.39.48
	♈ Aquarii	10.20.55	21.16	21.37.4	21.59	22.20	10.21.37.47	10.17.53.83
	♈ Pegasi	10.34. 7	34.28.3	34.49.6	35.11	35.32.4	10.34.49.05	10.31. 6.30
	♈ Aquarii	10.52.45	53. 6	53.27	53.48.4	54. 9.6	10.53.27.17	10.49.44.22
	♈ Pegasi	11.17.25	17.46	18. 7.3	18.29	18.50	11.18. 7.43	11.14.25.06
	♈ Aquarii	11.27.53	27.26.5	27.47.6	28. 9.3	28.30.3	11.27.47.8	11.24. 5.68
	♈ Pegasi	11.31. 8	31.27.5	31.48.5	32.10.3	32.31.5	11.31.48.68	11.28. 6.69
	♈ Pegasi	11.36.17.5	36.39.3	37. 0.5	37.22	37.44	11.37. 0.6	11.33.18.73
	♈ Pegasi	11.44.39.5	44.41.3	45. 3	45.25	45.46.6	11.55. 3.07	11.51.21.57

Date.	Stars.	1.	2.	Merid. wire.	3.	4.	5.	Mean Chronometer.	Clock.
		h. m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	h. m. s.	h. m. s.
10	α Equulei -	9.57.45	58. 6	58.27	58.48,5	59. 9,4	9.58.27,15	9.55.54,46	
	β Aquarii -	10.12.59	13.20	13.41	14. 2,5	14.23,7	10.13.41,2	10.11. 8,88	
	γ Pegasi -	10.26.10,8	26.32	26.53,3	27.15	27.36,2	10.26.53,43	10.24.21,36	
	δ Pegasi -	11. 9.28,3	9.49,6	10.10,5	10.32	10.53,2	11.10.10,68	11. 7.39,80	
	ϵ Aquarii -	11.19. 9	19.30	19.51,2	20.12,4	20.34	11.19.51,3	11.17.20,66	
	ζ Pegasi -	11.23. 9,4	23.31	23.52	24.13,7	24.35,2	11.23.52,22	11.21.21,70	
	η Pegasi -	11.28.21,3	28.42,5	29. 4	29.25,6	29.47,3	11.29. 4,12	11.26.33,72	
	α Pegasi -	11.46.23	46.44,6	47. 6,3	47.28,3	47.50,3	11.47. 6,47	11.44.36,50	
12	α Equulei -	9.49.47,4	50. 8,7	50.29,4	50.51	51.15,5	9.50.29,73	9.49.11,23	
	β Aquarii -	10. 5. 1,3	5.22,5	5.43,5	6. 5	6.26,2	10. 5.43,67	10. 4.25,60	
	γ Aquarii -	10.36.51,4	37.12,4	37.33,4	37.54,6	38.16	10.37.33,53	10.36.16,20	
	δ Pegasi -	11. 1.31,2	1.52,2	2.13,3	2.34,5	2.55,5	11. 2.13,33	11. 0.05,53	
	ϵ Aquarii -	11.11.11,5	11.33	11.53,7	12.15,3	12.36,4	11.11.53,93	11.10.37,38	
	ζ Pegasi -	11.15.12	15.33,4	15.54,5	16.16,4	16.37,5	11.15.54,72	11.14.38,28	
	η Pegasi -	11.20.23,6	20.45,1	21. 6,5	21.28,2	21.49,8	11.21. 6,65	11.19.50,34	
	α Pegasi -	11.38.25,5	38.47,4	39. 9	39.31	39.53	11.39. 9,15	11.37.53,33	
14	α Equulei -	9.41.54,6	42.15,7	42.37	42.58,3	43.19,4	9.42.37	9.42.28,05	
	β Pegasi -	10.10.20,2	10.41,5	11. 3	11.24,6	11.46,1	10.11. 3,07	10.10.54,92	
	γ Aquarii -	10.28.58,4	29.10,4	29.40,5	30. 1,7	30.23	10.29.40,58	10.29.32,78	
	δ Pegasi -	10.53.38,2	53.59,2	54.20,3	54.41,8	55. 3	10.54.20,47	10.54.13,42	
	ϵ Aquarii -	11. 3.18,6	3.40	4. 1	4.22,4	4.43,5	11. 4. 1,08	11. 3.54,32	
	ζ Pegasi -	11. 7.19	7.40,3	8. 1,9	8.23,4	8.45	11. 8. 1,92	11. 7.55,24	
	η Pegasi -	11.12.31	12.52,3	13.14	13.35,4	13.57	11.13.13,95	11.13. 7,40	
	α Pegasi -	11.30.32,7	30.54,4	31.16,2	31.38,3	32. 0	11.31.16,3	11.31.10,33	

Transits observed at CLIFTON.

Oct. 2	α Aquilæ -	6.46. 8	46.29	46.50,1	47.11,3	47.32,7	6.46.50,2	6.46.47,45
	β Aquilæ -	6.57.47,4	58. 8,7	58.30	58.51,7	59.13	6.58.30,13	6.58.27,38
	γ Aquilæ -	7.17.44,4	18. 5,5	18.26,5	18.47,6	19. 8,8	7.18.26,55	7.18.23,75
	δ Equulei -	8.22.23,4	22.44,4	23. 5,4	23.27	23.48	8.23. 5,6	8.23. 2,35
	ϵ Capricorni -	8.42.24	42.47	43. 9,1	43.32	43.54,5	8.43. 9,28	8.43. 5,98
	ζ Aquarii -	9.11.57	12.18,1	12.39,4	13. 0,5	13.21,7	9.12.39,35	9.12.35,95
	η Aquarii -	9.27.43,8	28. 5	28.25,7	28.47	29. 8,1	9.28.25,88	9.28.22,38
3	\odot 's { 1st limb	11.47.29	—	—	—	48.53,7	} 11.49.45,8	11.49. 6,5
	ad limb	—	—	50.20	50.41,2	51. 2,7		
	α Aquilæ -	6.42.10,3	42.31,4	42.52,7	43.14	43.35,3	6.42.52,73	6.42.40,48
	β Aquilæ -	6.53.50	54.11,4	54.32,4	54.54	55.15,5	6.54.32,62	6.54.20,37
	γ Aquilæ -	7.13.47	14. 8,1	14.29,1	14.50,5	15.11,7	7.14.29,25	7.14.16,85
	δ Aquarii -	7.49.34	49.55,5	50.16,9	—	51. 0	7.50.04,50	7.50.04,50
	ϵ Capricorni -	8. 5.41,6	6. 3,9	6.26,3	6.49,2	7.11,8	8. 6.26,52	8. 6.13,77
	ζ Equulei -	8.18.25,7	18.47	19. 8	19.29,4	19.50,4	8.19. 8,08	8.18.55,28
	η Capricorni -	8.38.26,9	38.49	39.11,8	39.34,2	39.56,9	8.39.11,77	8.38.58,87
	α Aquarii -	9. 8. 0	8.21	8.42	9. 3,1	9.24,2	9. 8.42,05	9. 8.29,15
	γ Aquarii -	9.23.46,3	24. 7,4	24.28,5	24.49,7	25.11	9.24.28,57	9.24.15,47
	η Aquarii -	9.37.29,2	37.50,3	38.11,2	38.32,4	38.53,8	9.38.11,35	9.37.58,10

Date.	Stars.	1.	2.	Mean wire 3.	4	5.	Mean Chronometer.	Clock.
5	☉'s { 1st limb	h. m. s.	m. s.	m. s.	m. s.	m. s.	h. m. s.	h. m. s.
	☉'s { 2d limb	11.46.50.7	47.12	47.33	47.51.5	48.15.7	11.48.17.54	11.48.8.64
	☉ Aquilæ -	6. —	—	34.59	35.2.05	35.11.5	6.34.59.05	6.34.27.35
	☉ Aquilæ -	6.45.56.4	46.17.7	46.39	47.0.5	47.21.8	6.46.39.05	6.46.7.22
	☉ Aquilæ -	7. 5.53.4	6.14.5	6.35.6	6.56.8	7.16	7. 6.35.65	7. 6.3.75
	☉ Aquarii -	7.41.40.6	42. 2	42.23.2	42.45	43. 1.3	7.42.23.18	7.41.51.28
	☉ Aquarii -	9. 0. 6	0.27.2	0.48	1. 0.1	1.30.5	9. 0.48.13	9. 0.15.68
	☉ Aquarii -	9.15.52.7	16.13.7	16.31.0	16.56	17.17	9.16.34.87	9.16. 2.27
	☉ Aquarii -	9.29.35.3	29.56.5	30.17.4	30.38.9	31. 0	9.30.17.2	9.29.41.92
	6	☉'s { 1st limb	11.46.30.5	46.51.9	47.13	47.54.1	47.55.9	11.48.17.66
☉'s { 2d limb		11.48.39.7	49. 1	49.22	49.43.4	50. 4.7		
☉ Aquilæ -		6.30.18.8	30.39.9	31. 1	31.2.3	31.43.4	6.31. 1.07	6.30.20.82
☉ Aquilæ -		6.41.58.3	42.19.5	42.40.7	43. 2.2	43.23.8	6.42.40.87	6.42. 0.57
☉ Aquilæ -		7. 1.55.4	2.16.3	2.37.5	2.58.7	3.19.9	7. 2.37.55	7. 1.57.15
☉ Aquarii -		7.37.44.4	38. 4	38.25.2	39.47	40. 8.1	7.38.25.31	7.37.44.75
☉ Equulei -		8. 6.33.8	6.55	7.16	7.37.4	7.58.7	8. 7.16.15	8. 6.35.35
☉ Capricorni		8.26.35.3	26.57.8	27.20	27.43	28. 5.5	8.27.20.27	8.26.39.37
☉ Aquarii -		8.56. 8	56.29.4	56.50.2	57.11.4	57.32.8	8.56.50.33	8.56. 9.23
☉ Aquarii -		9.11.54.8	12.16	12.36.9	12.58	13.19.2	9.12.36.9	9.11.55.72
☉ Aquarii -	9.25.37.7	25.58.7	26.19.5	26.41	27. 2	9.26.19.73	9.25.38.43	
8	☉'s { 1st limb	11.45.52.8	46.14	46.35	46.56.8	47.17.9	11.47.19.78	11.46.45.38
	☉'s { 2d limb	11.48. 1.8	48.23	48.44	49. 5.5	49.27		
	☉ Aquilæ -	6. —	—	—	43.27	43.48	6.23. 5.07	6.22. 8.92
	☉ Aquilæ -	6.53.58.9	54.20.5	54.41.4	55. 3	55.24	6.54.41.53	6.53.44.63
	☉ Aquarii -	7.29.47	—	30.3	—	31.13	7.30.30	7.29.32.8
	☉ Equulei -	7. —	—	—	59.42	60. 2.9	7.59.2.58	7.58.23.28
	☉ Capricorni	8.18.39.7	19. 2.2	19.24.5	19.4.2	20. 9.8	8.19.24.65	8.18.27.2
	☉ Aquarii -	8.48.12.7	48.33.5	48.54.7	49.16	49.37	8.48.54.77	8.47.57.07
	☉ Aquarii -	9.17.42	18. 3	18.24	18.45.4	19. 6.5	9.18.24.17	9.17.26.37
	☉ Aquarii -	—	—	—	—	—	—	—

Transits observed at ARBURY HILL.

Oct. 21	☉'s { 1st limb	11.42.46.8	43. 8.2	43.29.9	43.51.1	44.13	} 11.44.35.39	11.44.28.39
	☉'s { 2d limb	11.44.58	45.19.4	45.41	46. 2.5	46.24		
	☉ Aquilæ -	5.31. 5.5	31.26.5	31.47.7	32. 9	32.30.2		
	☉ Aquilæ -	5.42.44.5	43. 6	43.27.3	43.49	44.10.1		
25	☉ Aquilæ -	6. 2.42.4	3. 3.8	3.24.8	3.46	4. 7	} 5.31.47.73	5.31.39.53
	☉ Aquilæ -	5.15.17	15.38.4	15.59.3	16.21	16.42		
	☉ Aquilæ -	5.26.56.4	27.18	27.39	28. 0.6	28.22		
	☉ Aquilæ -	5.46.54.3	47.15.4	47.36.4	47.58	48.19		
25	☉'s { 1st limb	11.42. 0.5	42.22	42.43.6	43. 5.5	43.27	} 11.43.49.83	11.43.17.93
	☉'s { 2d limb	11.44.12.5	44.34.3	44.56	45.17.7	45.39.2		
	☉ Aquilæ -	5.12.19.5	11.40.6	12. 1.9	12.23.1	12.44.4		
	☉ Aquilæ -	5.24.58.5	23.20.3	23.41.4	24. 3	24.24.4		
25	☉ Aquilæ -	5.42.36.5	43.18	43.39	44. 0	44.21.2	} 5.23.41.58	5.23. 8.78
	☉ Aquilæ -	5.15.17	15.38.4	15.59.3	16.21	16.42		
	☉ Aquilæ -	5.26.56.4	27.18	27.39	28. 0.6	28.22		
	☉ Aquilæ -	5.46.54.3	47.15.4	47.36.4	47.58	48.19		

Transits observed at SHANKLIN FARM.

Date.	Stars.	1.	2.	Merid. wire.	3.	4.	5.	Mean Chronometer.	Clock.
		h. m. s.	m. s.	m. s.	m. s.	m. s.		h. m. s.	h. m. s.
1819. } May 10 }	Regulus -	6.51.42,2	52. 4	52.25,5	52.47,2	—		6.52.25,55	6.52.37,32
11	☉'s { 1st limb ad limb	11.58.51,7 0. 1. 5	59.14 1.27,3	59.36,2 1.49,6	59.58,5 2.11,8	60.20,5 2.34		0. 0.42,86	0. 0.49,49
12	d -	9. 10.41	11. 3,3	11.25,5	11.48	12.10,5		9.11.25,63	9.11.21,26
	♌ Virginis -	9. 37.45	38. 6,8	38.28	38.49,8	39.11,3		9.38.28,15	9.38.24,16
	♌ Virginis -	10. 0.10,7	0.32,2	0.53,5	1.15	1.36,5		10. 0.53,57	10. 0.49,30
	♌ Bootæ -	10.23. 6,5	23.28,7	23.51	24.13,2	24.35,5		10.23.50,98	10.23.46,29
	♌ Bootæ -	10.30.29,3	30.51,8	31.14	31.36,5	31.59		10.31.14,1	10.31. 9,37
	♌ Arcturus -	10.51.46	52. 9	53.31	53.58,8	53.16		10.52.31,13	10.52.26,25
13	☉'s { 1st limb ad limb Regulus -	11.58.44,6 0. 0.58,3 6.39.49	59. 7 1.20,5 40.11	59.29,2 1.43 40.32,5	59.51,8 2. 5,2 40.54,2	60.14 2.27,5 41.15,8		0. 0.36,11 6.40.32,5	0. 0.26,89 6.40.21,23
14	☉'s { 1st limb ad limb	11.58.41,8 0. 0.55,5	59. 4 1.18	59.26,5 1.40	59.48,8 2. 2,5	60.11,0 2.25		0. 0.33,31	0. 0.16,44
15	☉'s { 1st limb ad limb Regulus -	11.58.39,2 0. 0.53,4 6.31.53,8	59. 2 1.15,8 32.15,5	59.24 1.38,2 32.37	59.46,7 2. 0,5 32.58,7	60. 9 2.23 33.20,2		0. 0.31,18 6.32.37,03	0. 0. 6,51 6.32.10,35
16	☉'s { 1st limb ad limb Regulus - d - ♌ Virginis - ♌ Virginis - ♌ Bootæ - ♌ Bootæ - ♌ Arcturus -	11.58.37,5 0. 0.51,5 6.27.56,0 8.54.50,3 9.21.54,3 9.44.20 10. 7.15,7 10.14.38,7 10.55.55,4	59. 0 1.14 28.17,8 55.13 22.16 44.41,5 7.37,7 15. 1 36.18	59.22,5 1.36,5 28.39,5 55.35 22.37,3 45. 2,8 8. 0 15.23,3 36.40,2	59.45 1.59 29. 1 55.57,5 22.59,1 45.24,2 8.22,2 15.45,7 37. 3	60. 7 2.21,3 29.22,5 56.20 23.20,5 45.45,6 8.44,5 16. 8,2 37.25,2		0. 0.29,43 6.28.39,28 8.55.35,13 9.22.37,41 9.45. 2,82 10. 8. 0,02 10.15.23,37 10.36.40,33	11.59.57,71 6.28. 5,65 8.55. 0,74 9.22. 2,83 9.44.28,68 10. 7.25,28 10.14.48,58 10.36. 5,44

Comparisons of the Clock with the Chronometer.

Date.	Chronometer.	Clock.	Clock fast
	h. m. s.	h. m. s.	m. s.
July 22, P. M.	5.19.10,25	5.21.28	5.17,75
	6.10.40,8	6.16. 0	5.19,20
	9.53.20	9.58.47	5.27
	10.16.10	10.21.38	5.28
	10.31.31,5	10.40. 0	5.29,5
	21.49.45,5	21.55.37,3	5.51,8
	22.41. 0,25	22.46.51	5.53,75
	0.10.50,75	0.17.38	6.47,25
	9.17.40	9.25.38	7.58
	9.41. 0,8	9.49. 0	7.59,2
	10. 3.25,25	10.11.25	7.59,75
	10.22.20,5	10.30.21	8. 0,5
	0.13.28,5	0.22. 1	8. 0,5
	9.31.49,25	9.42. 0	10.10,75
August 5.	0.12. 6	0.22.23	10.17
	5.49.30,25	6. 0. 0	10.29,75
	9. 6.30	9.17. 7	10.37
	9.29.50	9.40.28	10.38
	9.51. 0,25	10. 1.39	10.38,75
	10.10. 5,5	10.20.45	10.39,5
	9. 4.40	9. 5.45	1. 2
	9.27.40,25	9.28.18	1. 2,75
	9.46.20	9.47.27	1. 3
	10.24.41	10.25.45	1. 4
	10.44. 0,5	10.45. 5	1. 4,5
	11. 5. 0	11. 6. 5	1. 5
	0.22.59,8	0.34.28	1.29,8
	8.38. 5,25	8.39.39	1.33,75
6	0.16.40,25	0.18.38	1.38,75
	0.16.45	0.19.18	2.33
	8.15.18,5	8.18. 0	2.41,5
	8.51. 0	8.53.47	2.47
	8.52.15,5	8.55. 5	2.47,5
	9.34.20,5	9.37. 9	2.48,7
	10.48.50,25	10.51.41	2.50,75
	0.16.15	0.20. 8	3.53
	5. 3.35,4	5. 7.37	4. 1,6
	8.22.20,4	8.26.38	4. 7,6
	8.44. 0,75	8.48. 9	4. 8,25
	9. 7. 0	9.11. 9	4. 9
	9.26.15,5	9.30.25	4. 9,5
	9.58.45,5	10. 2.56	4.10,5
10	10.40.10	10.44.22	4.12
	8.18. 0,5	8.22.52	4.51,5
	8.40.14,75	8.45. 7	4.52,25
	9. 8. 0	9. 9.53	4.53
11	9.23. 0,5	9.26.54	4.55,5

Date.	Chronometer.	Clock.	Clock fast.
	h. m. s.	h. m. s.	m. s.
August 11	9.54. 6,5	9.59. 1	4.54,5
	10.16.19,75	10.21.15	4.55,25
	10.36.50,25	10.41.46	4.55,75
12	0.15.45,4	0.21. 6	5.20,6
	4.57. 0,25	5. 2.29	5.28,75
	8.13.25,25	8.19. 0	5.34,75
	8.36. 0,5	8.41.36	5.35,5
	9. 1. 0,5	9. 6.37	5.36,5
	9.20.20	9.25.57	5.37
	9.50. 5	9.55.43	5.38
	10.11.55,5	10.17.34	5.38,5
	10.31.49,75	10.37.29	5.39,25
			Slow.
13	0.17.30,75	0.17. 8	0.22,75
	4.51. 0,5	4.50.46	0.14,5
	8.10.20,4	8.10.12	0. 8,4
			Fast.
14	0.15.51,5	0.16.12	0.20,5
	8. 6.25,25	8. 7. 0	0.34,75
	8.29.41,5	8.30.17	0.35,5
	8.51.20	8.51.56	0.36
	9.10.15,4	9.10.52	0.36,6
	10. 5.32,5	10. 6.11	0.38,5
	10.21.10	10.21.49	0.39
15	0.15.29,75	0.16.35	1. 5,25
	4.43.51,75	4.45. 5	1.15,25
	8. 3.54,75	8. 5.14	1.19,25
	8.24. 0	8.25.20	1.20
	8.47.15,25	8.48.36	1.20,75
	9. 6.15,5	9. 7.37	1.21,5
16	0.15. 0,25	0.16.49	1.48,75
	4.40.29,9	4.42.27	1.57,1
	7.58.56,75	8. 1. 0	2. 3,25
	8.43.19,5	8.45.24	2. 4,5
	9. 3.34,8	9. 5.40	2. 5,2
	10.13. 0,5	10.15. 8	2. 8,5
17	0.15.24,9	0.17.58	2.53,1
	7.56.41,12	7.59.28,5	2.47,38
	8.16. 5	8.18.53	2.48
	8.39.10,25	8.41.59	2.48,75
	8.58.11,5	9. 1. 1	2.49,5
	9.51.59,9	9.54.51	2.51,1
	10. 9.30,4	10.12.22	2.51,6
18	4.32.49,75	4.36.14	3.24,25
	8.12.34,8	8.16. 6	3.31,2
	8.35. 5	8.38.37	3.32
	8.54. 0,5	8.57.33	3.32,5
19	0.14.54,75	0.18.35	4. 0,25
			Slow.
31	7.47.49, 9	7.47:19	0.30,9

436 *Capt. KATER's experiments for determining the variation*

Date.	Chronometer.	Clock	Clock slow.
	h m s	h m s	m s
August 31	9 30 5,5	9 31 37	0 38,5
	10 41 39,75	10 41 12	0 27,75
	10 56 0,5	10 43 33	0 27,5
	11 16 0	11 15 33	0 27
	11 29 9,8	11 28 43	0 30,8
	11 40 40,5	11 40 11	0 29,5
September 2			1 1 1
	0 13 15,3	0 13 15	0 19,7
	9 45 0,8	9 45 32	0 41,2
	10 4 0,4	10 4 17	0 41,6
	10 33 49,75	10 34 22	0 42,25
	10 48 10,1	10 48 43	0 42,6
	11 19 41,75	11 20 14	0 43,25
	12 4 21,75	12 4 19	0 44,25
	4 9 38 4,75	9 39 15	1 30,25
	9 58 0,25	9 59 11	1 30,75
	10 24 45,5	10 26 17	1 31,5
	10 40 40,25	10 41 12	1 31,75
	10 53 0	10 54 32	1 32
	11 12 0,5	11 13 33	1 32,5
	11 31 0	11 32 33	1 33
	11 48 9,75	11 49 43	1 33,25
	11 56 5,5	11 57 19	1 33,5
	5 0 11 15,2	0 13 5	1 49,8
	6 0 11 49,8	0 14 12	2 22,2
			Slow
8	10 8 30	10 4 40	3 44
	10 28 5,5	10 24 42	3 43,5
	10 37 15,3	10 33 32	3 43,3
	10 55 49,9	10 52 7	3 42,9
	11 21 0,3	11 27 18	3 42,3
	11 40 9,8	11 36 28	3 41,8
	11 58 41,4	11 55 0	3 41,4
	10 6 0,5	10 3 28	2 52,5
	10 16 30,25	10 13 38	2 52,25
	10 30 5	10 27 33	2 52
10	11 13 30,8	11 11 0	2 50,8
	11 32 50,3	11 30 20	2 50,3
	11 50 9,9	11 47 40	2 29,9
	12 9 54 20,4	9 53 2	1 18,4
	10 8 50	10 7 32	1 18
	10 41 0,25	10 39 43	1 17,25
12	11 4 0,75	11 2 44	1 16,75
	11 23 40,25	11 22 24	1 16,25
	11 42 0,75	11 40 45	1 15,75
	9 45 4,9	9 44 56	0 8,9
	10 54 0,2	10 5 52	0 8,2
	10 17 40	10 17 32	0 8
14	10 32 4,75	10 31 57	0 7,75
	10 56 30	10 56 23	0 7
	11 15 26,5	11 15 20	0 6,5

Date.	Chronometer.	Clock.	Clock slow.
	h. m. s.	h. m. s.	m. s.
September 14	11.34.19,9	11.34.14	0. 5,9
October 2	6.49. 9,75	6.49. 7	0. 2,75
	7. 1. 2,75	7. 1. 0	0. 2,75
	7.21. 2,8	7.21. 0	0. 2,8
	8.27. 3,25	8.27. 0	0. 3,25
	8.48. 3,3	8.48. 0	0. 3,3
	9.15. 3,4	9.15. 0	0. 3,4
	9.31. 3,5	9.31. 0	0. 3,5
3 A. M.	11.54. 9,3	11.54. 0	0. 9,3
P. M.	6.45.12,25	6.45. 0	0.12,25
	6.57.12,25	6.57. 0	0.12,25
	7.16.12,4	7.16. 0	0.12,4
	7.54.12,5	7.54. 0	0.12,5
	8. 9.12,75	8. 9. 0	0.12,75
	8.22.12,8	8.22. 0	0.12,8
	8.42.12,9	8.42. 0	0.12,9
	9.11.30	9.11.17,1	0.12,9
	9.29.13,1	9.29. 0	0.13,1
	9.41.13,25	9.41. 0	0.13,25
5 A. M.	11.52.28,9	11.52. 0	0.28,9
P. M.	6.37.31,7	6.37. 0	0.31,7
	6.43.31,8	6.43. 0	0.31,8
	7. 9.31,9	7. 9. 0	0.31,9
	7.46.32	7.46. 0,1	0.32,1
	8. 1.32,25	8. 1. 0	0.32,25
October 5	9. 4.32,45	9. 4. 0	0.32,45
	9.18.32,6	9.18. 0	0.32,6
	9.32.32,7	9.32. 0	0.32,7
6 A. M.	11.51.37,3	11.51. 0	0.37,3
P. M.	6.32.40,25	6.32. 0	0.40,25
	6.44.40,3	6.44. 0	0.40,3
	7. 3.40,4	7. 3. 0	0.40,4
	7.40.40,6	7.40. 0	0.40,6
	8. 9.40,8	8. 9. 0	0.40,8
	8.29.40,9	8.29. 0	0.40,9
	8.59.41,1	8.59. 0	0.41,1
	9.14.41,25	9.14. 0	0.41,25
	9.28.41,3	9.28. 0	0.41,3
7 A. M.	11.51.45,6	11.51. 0	0.45,6
8 A. M.	11.51.54,4	11.51. 0	0.54,4
P. M.	6.24.56,75	6.24. 0	0.56,75
	6.57.56,9	6.57. 0	0.56,9
	7.33.57,2	7.33. 0	0.57,2
	8. 1.57,3	8. 1. 0	0.57,3
	8.21.57,45	8.21. 0	0.57,45
	8.50.57,7	8.50. 0	0.57,7
	9.22. 57,8	9.22. 0	0.57,8
21 A. M.	11.49. 7	11.49. 0	0. 7
P. M.	5.35. 8,2	5.35. 0	0. 8,2
	5.46. 8,2	5.46. 0	0. 8,2

438 *Capt. KATER's experiments for determining the variation*

Date.	Chronometer.	Clock.	Clock slow.
	h. m. s.	h. m. s.	m. s. .
October 21	6. 6. 8,25	6. 6. 0	0. 8,25
	6.19. 8,25	6.19. 0	0. 8,25
October 25	5.19.28,75	5.19. 0	0.28,75
	5.30.28,75	5.30. 0	0.28,75
	5.50.28,8	5.50. 0	0.28,8
	6.55.28,95	6.55. 0	0.28,95
	7.10.29,05	7.10. 0	0.29,05
	7.44.29,2	7.44. 0	0.29,2
	7.59.29,2	7.59. 0	0.29,2
	8.14.29,25	8.14. 0	0.29,25
26 A. M.	11.48.31,9	11.48. 0	0.31,9
P. M.	5.15.32,8	5.15. 0	0.32,8
	5.26.32,8	5.26. 0	0.32,8
	5.50.32,9	5.50. 0	0.32,9
			Fast.
1819. May 10	6.56.48,25	6.57. 0	0.11,75
11	0. 4.53,4	0. 5. 0	0. 6,6
			Slow.
12	8.28. 4,25	8.28. 0	0. 4,25
	9.16. 4,4	9.16. 0	0. 4,4
	9.46. 4,4	9.46. 0	0. 4,4
	10. 4. 4,5	10. 4. 0	0. 4,5
	10.34. 4,75	10.34. 0	0. 4,75
	10.55. 4,9	10.55. 0	0. 4,9
13	0. 5. 9,25	0. 5. 0	0. 9,25
	6.45.11,3	6.45. 0	0.11,3
14	0. 4.16,9	0. 4. 0	0.16,9
15	0. 4.24,7	0. 4. 0	0.24,7
	6.35.26,7	6.35. 0	0.26,7
16	0. 4.31,75	0. 4. 0	0.31,75
	6.31.33,75	6.31. 0	0.33,75
	8.57.34,4	8.57. 0	0.34,4
	9.25.34,6	9.25. 0	0.34,6
	9.47.34,75	9.47. 0	0.34,75
	10.17.34,8	10.17. 0	0.34,8
	10.38.34,9	10.38. 0	0.34,9

Observations for the error of the Chronometer.

UNST, 1818, 23d July, P. M. ☉'s U. L.

Chronometer.	Level.	Readings, &c.
h. m. s.		
4.21.24,5	+ 7 — 3	First Vernier - - - 239.32.15
4.24.29,5	+ 5 — 5	Second - - - 5
4.26.46,0	+ 10 + 4	Third - - - 5
4.28. 1,5	+ 9 + 0	Fourth - - - 10
Mean - - - 4.25.10 4	+ 31 — 4	Mean - - - 239.32. 8,7
True time - - - 4.24.23,3		Level - - - + 32,4
Chron. fast - - - 47,1		Index - - - + 18,0
		4) 239.32.59,1
		Observed Z. D. - - 59.53.14,8
		Ref. and Parall. - - + 1.32,3
		Semidiam. - - + 15.46,4
		True Z. D. - - - 60.10.33,5
$\frac{(+31-4)}{2} \times 2,4 = +32,4$		

UNST, 23rd July, P. M. ☉'s U. L.

Chronometer.	Level.	Readings, &c.
h. m. s.		
4.31.55	+ 6 — 1	First Vernier - - - 243.58.10
4.33.16	+ 4 — 2	Second - - - 58. 0
4.35.32,5	+ 9 + 4	Third - - - 57.50
4.37. 0,5	+ 5 + 0	Fourth - - - 58.15
Mean - - - 4.34.26,0	+ 24 + 1	Mean - - - 243.58.3,7
True time - - - 4.33.37,9		Level - - - + 30,0
Chron. fast - - - 48,1		Index - - - + 18,0
		4) 243.58.51,7
		Observed Z. D. - - 60.59.42,9
		Ref. and Parall. - - + 1.36,8
		Semidiam. - - + 15.46,4
		True Z. D. - - - 61.17. 6,1
$\frac{(+24+1)}{2} \times 2,4 = +30,0$		

From the mean of the above observations, the chronometer appears to be 47,6 too fast, and the rate being —1,81, we have 47,9 for its error too fast at noon.

440 *Capt. KATER's experiments for determining the variation*

PORTNOY, 1818, 3rd August, P. M. ☉ S U I.

Chronometer.	Level.	Readings, &c.
h. m. s.		
4. 8.35	+ 4 - 2	First Vernier - 234 20,5
4. 9.50,5	- 1 - 6	Second - - - 15
4.11. 7	+ 2 - 4	Third - - - 30
4.12. 4,5	+ 5 - 0	Fourth - - - 60
Mean - - - 4.10.24,2	+10 -12	Mean - - - 234 26,50
True time - - - 4. 2.31,3		Level - - - 2,4
Chron. fast - - - 7 52,9		Index - - - 18,0
		4) 234 27 5,6
		Observed Z. D. - 58 36 46,4
		Ref. and Parall. - + 1 24,7
		Semidiam. - + 15 47
		True Z. D. - 58 53 58,1
$\frac{(10-12)}{2} \times 2,4 = -2,4$		

PORTNOY, 3rd August, P. M. ☉ S U I.

Chronometer.	Level.	Readings, &c.
h. m. s.		
4.15.56,5	+ 4 + 2	First Vernier - 238 18 40
4.17. 9	+ 4 + 1	Second - - - 25
4.18.29,5	+ 5 + 3	Third - - - 30
4.20. 5,5	+ 1 - 2	Fourth - - - 50
Mean - - - 4.17.55,1	+14 + 4	Mean - - - 238 18 36,6
True time - - - 4.10. 3,4		Level - - - 21,6
Chron. fast - - - 7.51,7		Index - - - 18,0
		4) 238 19 16,2
		Observed Z. D. - 59 34.49,0
		Ref. and Parall. - + 1.30,4
		Semidiam. - + 15.47
		True Z. D. - 59.51. 6,4
$\frac{(+14+4)}{2} \times 2,4 = 21,6$		
		Sun rather obscure during some of these observations.

From the mean of the above observations, the chronometer appears to be 7^m.52^s.8 too fast, and the rate being — 1^s.7 we have the chronometer 7^m.52^s.58 too fast at apparent noon.

LEITH FORT, 1818, 17th September, A. M. ☉'s U. L.

Chronometer.	Level.	Readings, &c.
h. m. s.		
7.36.34	+24—17	First Vernier - 297.25.25
7.38.26	+22—19	Second - - 25.40
7.41.16	+20—21	Third - - 25.25
7.43. 1	+22—15	Fourth - - 25.15
Mean - - 7.39.49,2	+88—72	Mean - - 297.25.26,2
True time - 7.31. 7,6		Level - - + 19,2
Chron. fast - 8.41,6		Index - - + 18
		4) 297.26.3,4
		Observed Z. D. 74.21.30,85
		Ref. and Parall. + 3.16,6
		Semidiam. - + 15.57,3
		True Z. D. - 74.40.44,75
$\frac{(+88-72)}{2} \times 2,4 = +19,2$		

LEITH FORT, 17th September, A. M. ☉'s U. L.

Chronometer.	Level.	Readings, &c.
h. m. s.		
7.49. 4	+20—20	First Vernier - 291.19.55
7.50.19	+18—20	Second - - 19.45
7.52.19,5	+25—13	Third - - 19.55
7.54.16,5	+16—22	Fourth - - 19.55
Mean - - 7.51.29,8	+79—75	Mean - - 291.19.52,5
True time - 7.42.48 2		Level - - + 4,8
Chron. fast - 8.41,6		Index - - + 18
		4) 291.20.15,3
		Observed Z. D. 72.50. 3,85
		Ref. and Parall. + 2.58
		Semidiam. - - + 15.57,3
		True Z. D. - 73. 8.59,15
$\frac{(+79-75)}{2} \times 2,4 = +4,8$		

From the above observations the chronometer appears to be 8^m.41,6 too fast, and the rate being—1',85, we have the chronometer 8^m.43',18 too fast at apparent noon.

SHANKLIN FARM, 10th May, 1819, A. M. O's U. L.

Chronometer.		Level.	Readings, &c.	
	h. m. s.			
	8.39.32	+18—15	First Vernier	- 204.40.35
	8.41. 3	+11—19	Second	- 20
	8.42.56	+12—19	Third	- 10
	8.44.44	+13—17	Fourth	- 30
Mean	- 8.42. 3.75	+54—70	Mean	- 204.40.23,7
True time	- 8.37.24,90		Level	- 19,2
Chron. fast	- 4.38,85		Index	- + 13,0
				4) 204.40.17,5
			Observed Z. D.	- 51.10. 4,35
			Ref. and Parall.	+ 1. 5,0
			Semidiam.	- + 15.51,4
			True Z. D.	- 51.27. 0,75
$\frac{(+54-70)}{2} \times 2,4 = -19,2$				

SHANKLIN FARM, 10th May, A. M. O's U. L.

Chronometer.		Level.	Readings, &c.	
	h. m. s.			
	8.50.33	+20—10	First Vernier	- 197.31.50
	8.52.14	+17—13	Second	- 35
	8.56.23	+22— 5	Third	- 30
	8.57.57	+14—14	Fourth	- 50
Mean	- 8.54.16,75	+73—42	Mean	- 197.31.41,2
True time	- 8.49.36,90		Level	- + 37,2
Chron. fast	- 4.39,85		Index	- + 13,0
				4) 197.32.31,4
			Observed Z. D.	- 49.23. 7,85
			Ref. and Parall.	+ 1. 1,2
			Semidiam.	- + 15.51,4
			True Z. D.	- 49.40. 0,45
$\frac{(+73-42)}{2} \times 2,4 = +37,2$				

444 *Capt. KATER's experiments for determining the variation*

SHANKLIN FARM, 10th May, A. M. O's U. L.

Chronometer.		Level.		Readings, &c.	
	h. m. s.				
	9.3. 1	+21	— 6	First Vernier	- 191.30. 0
	9.4. 9	+ 9	— 20	Second	- 29.30
	9.5.37	+21	— 6	Third	- 29.40
	9.6.36	+20	— 6	Fourth	- 30. 0
Mean	- 9.4.50,75	+71	— 38	Mean	- 191.29.47,5
True time	- 9.0.10,40			Level	- + 39,6
Chron. fast.	- 4.40,35			Index	- + 13,0
					4) 191.30.40,1
				Observed Z. D.	- 47.52.40,05
				Ref. and Parall.	+ 58,5
				Semidiam.	- + 15.51,4
				True Z. D.	- 48. 9.30,0
$\frac{(+71-38)}{2} \times 2,4 = +39,6$					

Observations of Coincidences.

1818, June 13, A. M. at Mr. Browne's house, Portland-place, LONDON. Barometer 29.9 inches, clock gaining 1'.5 in a mean solar day.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
71.2	h. m. s.	°	°				s.	
	10. 5. 9	1.38	1.35	489			2.98	
	13.18	1.33	1.30	490			2.77	
	21.28	1.27	1.25	491			2.56	
	29.39	1.23	1.20	490			2.36	
	37.49	1.17	1.15	491			2.17	
	46. 0	1.13	1.10	491			1.98	
	54.11	1.08	1.05	490			1.80	
	11. 2.21	1.03	1.00	493			1.64	
	10.34	0.98	0.96	490			1.51	
	19.44	0.95	0.93	490			1.42	
72.0	26.56	0.92						
71.6	Mean			490.5	489.5	86049.20	2.12	86051.32

June 14, A. M.

Barometer 30.0 inches.

69.3	10.27.30	1.39	1.36	490			3.03	
	35.40	1.33	1.30	491			2.77	
	43.51	1.28	1.25	491			2.56	
	52. 2	1.23	1.20	490			2.36	
70.3	11. 0.12	1.17	1.15	492			2.17	
	8.24	1.13	1.10	491			1.98	
	16.35	1.08	1.05	492			1.80	
	24.47	1.03	.01	491			1.67	
	32.58	0.99	0.97	492			1.54	
	41.10	0.96	0.94	493			1.45	
70.6	49.23	0.92						
70.1	Mean			491.8	489.3	86049.77	2.13	86051.90

446 *Capt. KATER's experiments for determining the variation*

June 15, A. M. LONDON.

Barometer 30.05 inches, clock gaining 1'.5.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
^a 69.6	h. m. s. 10. 3.10 11.20 19.31 27.42 35.54 44. 5 52.17	° 1.34 1.29 1.23 1.18 1.13 1.09 1.05	° 1.31 1.26 1.20 1.15 1.11 1.07	490 491 491 492 491 492			s. 2.81 2.60 2.36 2.17 2.02 1.88	
69.9	Mean			491.17	489.17	86049.68	2.31	86051.99

June 16, A. M.

Barometer 29.95 inches.

70.3	9.58.13 10. 6.24 14.34 22.46 30.57 39. 9 47.21 55.33 11. 3.45 11.58 20.10	1.26 1.22 1.16 1.12 1.06 1.02 0.98 0.94 0.90 0.87 0.82	1.24 1.18 1.14 1.09 1.04 1.00 0.96 0.92 0.88 0.84	491 490 492 491 492 492 492 492 492 492 492			2.52 2.28 2.13 1.95 1.77 1.64 1.51 1.39 1.27 1.15	
70.5	Mean			491.7	489. 7	86050.06	1.76	86051.82

July 23, P. M. at UNST.

Clock gaining 50'.63 in a mean solar day. Barometer 30.0 inches.

Temp.	Time of Coin- cidence.	Arc of vibra- tion.	Mean Arc.	Interval in Seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correc- tion for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				s.	
58.0	0.44.47	1.25	1.21	481			2.40	
	52.48	1.18	1.16	481			2.20	
	1. 0.49	1.14	1.11	483			2.02	
	8.52	1.08	1.05	482			1.80	
	16.54	1.03	1.00	483			1.64	
	24.57	0.98	0.95	482			1.48	
	32.59	0.93	0.91	483			1.36	
	41. 2	0.90	0.87	483			1.24	
	49. 5	0.85	0.83	483			1.13	
	57. 8	0.82	0.80	482			1.05	
58.8	2. 5.10	0.78						
58.4	Mean			482.3	480.3	86092.15	1.63	86093.78

July 23, P. M.

Barometer 30.3 inches.

58.8	2. 9.54	1.21	1.18	481			2.28	
	17.55	1.16	1.13	481			2.10	
	25.56	1.11	1.08	481			1.91	
	33.57	1.06	1.03	481			1.74	
	41.58	1.01	0.99	482			1.61	
	50. 0	0.97	0.95	481			1.48	
	58. 1	0.94	0.92	482			1.39	
	3. 6. 3	0.90	0.87	482			1.24	
	14. 5	0.85	0.83	482			1.13	
	22. 7	0.82	0.80	482			1.05	
59.8	30. 9	0.79						
59.3	Mean			481.5	479.5	86091.55	1.59	86093.14

448 Capt. KATER's experiments for determining the variation

July 24, A. M. UNST.

Clock gaining 50'.63.

Barometer 29.9 inches.

Temp.	Time of Coin- cidence.	A o of vibra- tion.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correc- tion for Arc.	Vibrations in 24 hours.
56.7	h. m. s. 8. 4.15 12.16 20.17 28.18 36.20 44.21 52.22 9. 0.24 8.27 16.29 24.32	1.22 1.17 1.12 1.08 1.03 0.98 0.94 0.91 0.87 0.83 0.80	1.19 1.14 1.10 1.05 1.00 0.96 0.92 0.89 0.85 0.81	481 481 481 482 481 481 482 483 482 483			s. 2.32 2.13 1.99 1.80 1.64 1.51 1.39 1.30 1.18 1.08	
58.0	24.32	0.80	0.81	483				
57.3	Mean			481.7	479.7	86091.70	1.63	86093.33

July 24, P. M.

Barometer, 29.82 inches.

59.7	1. 2.36 10.37 18.36 26.37 34.37 42.38 50.39 58.39 2. 0.40 14.41 22.42	1.21 1.14 1.10 1.05 1.03 0.99 0.97 0.93 0.89 0.85 0.83 0.79	1.17 1.12 1.07 1.03 0.99 0.95 0.91 0.87 0.84 0.81	481 479 481 480 481 481 480 481 481 481 481			2.24 2.06 1.88 1.74 1.60 1.48 1.36 1.24 1.16 1.08	
59.8	22.42	0.79	0.81	481				
59.7	Mean			480.6	478.6	86090.87	1.58	86092.45

in the length of the pendulum vibrating seconds.

449

July 25, A. M. UNST.

Clock gaining 50'.63.

Barometer 29.84 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
57.00	h. m. s.	°	°					
	7.41.34	1.20	1.17	481			2.24	
	49.35	1.14	1.12	481			2.06	
	57.36	1.10	1.07	481			1.88	
	8. 5.37	1.05	1.03	482			1.74	
	13.39	1.01	0.99	481			1.60	
	21.40	0.97	0.95	482			1.48	
	29.42	0.93	0.91	482			1.36	
	37.44	0.89	0.87	482			1.24	
	45.46	0.85	0.83	481			1.13	
	53.47	0.82	0.80	482			1.05	
58.4	9. 1.49	0.79						
57.7	Mean			481.5	479.5	86091.54	1.58	86093.12

July 25, P. M.

Barometer 29.72 inches.

58.7	2. 0.27	1.21	1.19	480			2.32	
	8.27	1.17	1.14	480			2.13	
	16.27	1.11	1.08	480			1.91	
	24.27	1.06	1.03	479			1.74	
	32.26	1.01	0.99	481			1.60	
	40.27	0.98	0.95	480			1.48	
	48.27	0.93	0.91	480			1.36	
	56.27	0.90	0.88	482			1.27	
59.3	3. 4.29	0.87	0.85	480			1.18	
	12.29	0.83						
59.0	Mean			480.2	478.2	86090.57	1.67	86092.24

450 *Capt. KATER's experiments for determining the variation*

July 26, A. M. UNST:

Clock gaining 50'.63.

Barometer 29.95 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
57,1	h. m. s.	°	°				s.	
	7.42.47	1,13	1,10	480			1,98	
	50.47	1,08	1,06	480			1,84	
	58.47	1,04	1,02	481			1,71	
	8. 6.48	1,00	0,98	481			1,57	
	14.49	0,97	0,95	481			1,48	
	22.50	0,93	0,91	480			1,36	
	30 50	0,89	0,86	481			1,21	
	38.51	0,84	0,82	481			1,10	
	46.52	0,81	0,80	481			1,05	
	54.53	0,79	0,77	481			0,97	
58,6	9. 2.54	0,75						
47,8	Mean			480,7	478,7	86090,94	1,43	86092,37

Oil was now applied to the scapement without stopping the clock.

in the length of the pendulum vibrating seconds. 451

July 27, A. M.—UNST.

Clock gaining 50',63.

Barometer 29,95 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
56,4	h. m. s.	°	°				s.	
	7.45.32	1,14	1,11	479			2,02	
	53.31	1,09	1,06	480			1,84	
	8. 1.31	1,04	1,02	478			1,71	
	9.29	1,00	0,98	480			1,57	
	17.29	0,97	0,94	480			1,45	
	25.29	0,92	0,90	480			1,33	
	33.29	0,88	0,86	480			1,21	
	41.29	0,84	0,82	481			1,10	
	49.30	0,80	0,78	480			1,00	
	57.30	0,77	0,75	480			0,92	
57,3	9. 5.30	0,73						
56,8	Mean			479,8	477,8	86090,27	1,42	86091,69

July 27, P. M.

Barometer 30,0 inches.

57,2	1.29. 6	1,18	1,15	479			2,17	
	37. 5	1,13	1,10	478			1,98	
	45. 3	1,08	1,06	478			1,84	
	53. 1	1,04	1,01	480			1,68	
	2. 1. 1	0,98	0,97	481			1,54	
	9. 2	0,96	0,94	479			1,45	
	17. 1	0,92	0,90	480			1,33	
	25. 1	0,88	0,86	480			1,21	
	33. 1	0,84	0,83	480			1,13	
	41. 1	0,82	0,80	480,5			1,05	
57,2	49.01,5	0,78						
57,3	Mean			479,6	477,6	86090,08	1,54	86091,62

452 Capt. KATER's experiments for determining the variation

July 28, A. M.—UNST.

Clock gaining 50^s.63.

Barometer 30.05 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				s.	
53.8	8. 2. 7	1.29	1.21	480			2.40	
	10. 7	1.13	1.10	480			1.98	
	18. 7	1.08	1.05	480			1.80	
	26. 7	1.03	1.02	480			1.70	
	34. 7	1.01	0.98	481			1.57	
	42. 8	0.96	0.94	481			1.15	
	50. 9	0.92	0.90	480			1.33	
	58. 9	0.88	0.86	482			1.21	
	9. 6.11	0.85	0.83	481			1.13	
	14.12	0.82	0.80	482			1.05	
54.8	22.14	0.78						
54.8	Mean			480.7	478.7	86090.95	1.56	86092.51

July 28, P. M.

Barometer 30.2 inches.

57.6	2.12. 1	1.37	1.24	478			2.52	
	19.50	1.33	1.19	478			2.32	
	27.57	1.17	1.14	478			2.13	
	35.55	1.11	1.08	480			1.91	
	43.55	1.06	1.04	478			1.77	
	51.59	1.02	1.00	480			1.61	
	3. 9.53	0.98	0.95	479			1.48	
	7.52	0.93	0.91	481			1.36	
	15.53	0.89	0.87	480			1.24	
	23.53	0.85	0.83	480			1.13	
58.5	32.53	0.82						
58.0	Mean			479.2	477.2	86089.82	1.75	86091.57

August 6, A. M. at PORTSOY.—1st Series.

Clock gaining 37'.63 in a mean solar day. Barometer 29.95 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
64.8	h. m. s.	°	°				s.	
	7.38.22	1.16	1.14	488			2.13	
	46.30	1.12	1.09	489			1.95	
	54.39	1.07	1.05	488			1.80	
	8. 2.47	1.03	1.01	489			1.67	
	10.56	0.99	0.96	489			1.51	
	19. 5	0.94	0.92	489			1.39	
	27.14	0.90	0.88	489			1.27	
	35.23	0.87	0.85	490			1.18	
	43.33	0.84	0.82	489			1.10	
	51.42	0.80	0.78	489			1.00	
	59.51	0.76						
64.8	Mean			488.9	486.9	86084.03	1.50	86085.53

August 6, P. M. Barometer 30.0 inches.

64.8	1. 0.42	1.23	1.19	486			2.32	
	8.48	1.16	1.14	486			2.13	
	16.54	1.12	1.08	487			1.91	
	25. 1	1.05	1.03	487			1.74	
	33. 8	1.02	1.00	487			1.64	
	41.16	0.98	0.95	487			1.48	
	49.22	0.93	0.91	487			1.36	
	57.29	0.89	0.87	487			1.24	
	2. 5.36	0.85	0.84	487			1.16	
	13.43	0.83	0.81	488			1.07	
	21.51	0.79						
65.7	Mean			486.9	484.9	86082.58	1.61	86084.19

454 *Capt. KATER's experiments for determining the variation*

August 7, A. M. PORTSOY.—1st Series.

Clock gaining 37'.63.

Barometer 29.89 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correc- tion for Arc.	Vibrations in 24 hours.
62.7	h. m. s.	°	°				s.	
	7.26.17	1.15	1.12	485			2.06	
	34.22	1.10	1.08	485			1.91	
	42.27	1.06	1.03	485			1.74	
	50.32	1.01	0.99	486			1.61	
	58.38	0.98	0.96	486			1.51	
	6. 6.41	0.94	0.91	485			1.36	
	14.49	0.89	0.87	486			1.24	
	22.55	0.85	0.84	486			1.16	
	31. 1	0.83	0.81	486			1.08	
	39. 7	0.79	0.77	486			0.97	
61.9	47.13	0.76						
62.3	Mean			485.6	483.6	86081.65	1.46	86083.09

August 7, P. M.

Barometer 29.88 inches.

62.2	0.52. 9	1.19	1.16	483			2.21	
	1. 0.12	1.14	1.11	484			2.02	
	8.16	1.09	1.07	484			1.87	
	16.20	1.06	1.02	484			1.71	
	24.24	1.00	0.98	485			1.57	
	32.29	0.96	0.94	485			1.45	
	40.34	0.93	0.91	484			1.36	
	48.38	0.89	0.87	485			1.24	
	56.43	0.85	0.83	484			1.12	
	2. 4.47	0.82	0.80	486			1.05	
63.0	12.53	0.78						
62.6	Mean			484.4	482.4	86080.74	1.56	86082.30

August 8, A. M. PORTSOY.—1st. Series.

Clock gaining 37'.63.

Barometer 30.05 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				s.	
58.6	7.39.46	1.21	1.18	482			2.28	
	47.48	1.15	1.13	483			2.10	
	55.51	1.12	1.09	483			1.95	
	8. 3.54	1.06	1.04	484			1.77	
	11.58	1.02	1.00	483			1.64	
	20. 1	0.98	0.96	484			1.51	
	28. 5	0.94	0.92	483			1.39	
	36. 8	0.90	0.88	484			1.27	
	44.12	0.86	0.84	484			1.16	
	52.16	0.83	0.81	484			1.08	
59.0	9. 0.20	0.79						
58.8	Mean			483.4	481.4	86080.00	1.61	86081.61

August 8, P. M.

Barometer 30.09 inches.

60.0	1. 4.28	1.16	1.13	483			2.10	
	12.31	1.11	1.08	482			1.91	
	19.33	1.06	1.04	482			1.77	
	27.35	1.02	1.00	483			1.64	
	36.38	0.94	0.96	483			1.51	
	44.41	0.94	0.92	483			1.39	
	52.54	0.90	0.88	484			1.27	
	2. 0.48	0.86	0.84	483			1.16	
	8.51	0.83	0.81	483			1.08	
	16.54	0.79	0.77	483			0.97	
61.1	24.57	0.76						
60.5	Mean			482.9	480.9	86079.63	1.48	86081.11

Oil was applied to the scapement without stopping the clock.

456 Capt. KATER's experiments for determining the variation

August 9, A. M. PORTSOY.—1st Series.

Clock gaining 37'.63.

Barometer 30.04 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
60.8	h. m. s.	°	°				s.	
	7.34.10	1.17	1.15	479			2.17	
	42. 9	1.13	1.10	480			1.98	
	50. 9	1.08	1.06	480			1.84	
	58. 9	1.04	1.02	481			1.71	
	8. 6.10	1.00	0.97	481			1.54	
	14.11	0.95	0.93	480			1.42	
	22.11	0.92	0.90	481			1.33	
	30.12	0.88	0.86	481			1.21	
	38.13	0.84	0.82	480			1.10	
	46.13	0.80	0.78	482			0.99	
60.5	54.15	0.77						
60.4	Mean			481.5	479.5	86078.60	1.53	86080.13

August 9. P. M.

Barometer 30.04 inches.

60.8	0.55.12	1.21	1.18	479			2.29	
	1. 3.11	1.16	1.13	477			2.09	
	11. 9	1.10	1.08	479			1.91	
	19. 8	1.06	1.03	480			1.74	
	27. 8	1.01	0.99	479			1.61	
	35. 7	0.98	0.95	480			1.48	
	43. 7	0.93	0.91	480			1.36	
	51. 7	0.90	0.88	480			1.27	
	59. 7	0.86	0.84	480			1.16	
	2. 7. 7	0.82	0.80	480			1.05	
60.7	15. 7	0.79						
60.5	Mean			479.4	477.4	86077.03	1.60	86078.63

August 10, A. M. PORTSOY.—1st. Series.

Clock gaining 37'.63.

Barometer 30.10 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
58.7	h. m. s. 7.30.24	1.20	1.18	478			s. 2.29	
	38.22	1.16	1.13	478			2.09	
	46.20	1.10	1.08	479			1.91	
	54.19	1.06	1.03	480			1.74	
	8. 2.19	1.01	0.98	479			1.57	
	10.18	0.96	0.94	479			1.45	
	18.17	0.93	0.91	480			1.36	
	26.17	0.89	0.87	479			1.24	
	34.16	0.85	0.83	479			1.13	
	42.15	0.82	0.81	480			1.08	
59.0	50.15	0.80						
58.8	Mean			479.1	477.1	86076.80	1.59	86078.39

August 10, P. M.

Barometer 30.16 inches.

59.8	1.26.11	1.18	1.15	477			2.17	
	31. 8	1.13	1.10	478			1.98	
	42. 6	1.08	1.06	478			1.84	
	50. 4	1.04	1.01	478			1.67	
	58. 2	0.99	0.97	478			1.54	
	2. 6. 0	0.95	0.93	478			1.42	
	13.58	0.91	0.89	478			1.30	
	22.56	0.87	0.85	480			1.18	
	30.56	0.84	0.82	478			1.10	
	38.54	0.81	0.79	478			1.03	
60.8	45.52	0.79						
60.3	Mean			478.1	476.1	86076.04	1.52	86077.56

458 Capt. KATER's experiments for determining the variation

August 11. A. M. PORTSOV.—1st Series.

Clock gaining 37'.63.

Barometer 30.28 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
56.3	h. m. s.	°	°				s.	
	7.35.17	1.20	1.17	478			2.21	
	43.15	1.14	1.12	479			2.06	
	51.14	1.10	1.07	479			1.88	
	59.13	1.04	1.02	479			1.71	
	8. 7.12	1.01	0.98	479			1.57	
	15.11	0.96	0.94	479			1.45	
	23.10	0.92	0.90	480			1.33	
	31.10	0.89	0.87	479			1.24	
	39. 9	0.85	0.83	480			1.13	
	47. 9	0.81	0.79	480			1.02	
57.0	55. 9	0.78						
56.6	Mean			479.2	477.2	86076.88	1.56	86078.44

August 11, P. M.

Barometer 30.27 inches.

59.5	1.38.11	1.18	1.15	478			2.17	
	46. 9	1.13	1.10	477			1.98	
	54. 6	1.08	1.06	478			1.84	
	2. 2. 4	1.04	1.02	477			1.77	
	10. 1	1.00	0.98	478			1.57	
	18.59	0.96	0.93	478			1.42	
	26.57	0.91	0.88	478			1.30	
	34.55	0.87	0.85	478			1.18	
	42.53	0.83	0.82	478			1.10	
	50.51	0.80	0.79	478			1.03	
60.5	58.49	0.78						
60.0	Mean			477.8	475.8	86075.81	1.53	86077.34

August 12, A. M. PORTSOY.—1st Series.

Clock gaining 37^s.63

Barometer 30.26 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
58.6	h. m. s. 7.31.30 39.27 47.24 55.21 8. 3.18 11.16 19.13 27.12 35. 9 43. 7 51. 5 59. 2	1.16 1.11 1.06 1.01 0.97 0.94 0.90 0.86 0.82 0.79 0.76 0.73	1.13 1.08 1.03 0.99 0.95 0.92 0.88 0.84 0.80 0.77 0.74	477 477 477 477 478 477 479 477 478 478 477			s. 2.09 1.91 1.74 1.60 1.48 1.39 1.27 1.16 1.05 0.97 0.89	
59.2	Mean			477.4	475.4	86075.51	1.41	86076.92

August 12, P. M.

Barometer 30.27 inches.

61.0	1. 9.25 17.21 25.17 33.19 41.10 49. 6 57. 3 2. 5. 0 12.58 20.54 28.52	1.18 1.13 1.08 1.03 1.00 0.95 0.91 0.87 0.84 0.81 0.78	1.15 1.11 1.06 1.01 0.97 0.93 0.89 0.85 0.82 0.79	476 476 476 477 476 477 477 478 476 478			2.17 2.02 1.84 1.67 1.54 1.42 1.30 1.18 1.10 1.02	
61.3	Mean			476.7	474.7	86074.98	1.53	86076.51

August 13, P. M. PORTSOY.—2d Series.

Clock gaining 42^s.18 in a mean solar day. Barometer 30.25 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
61.5	h. m. s.	°	°				s.	
	1.10.17	1.21						
	18.13	1.16	1.18	476			2.28	
	26. 9	1.10	1.13	476			2.10	
	34. 6	1.06	1.08	477			1.91	
	42. 2	1.01	1.03	476			1.74	
	49.58	0.97	0.99	476			1.61	
	57.55	0.93	0.95	477			1.48	
	2. 5.52	0.90	0.91	477			1.35	
	13.48	0.86	0.88	476			1.27	
	21.46	0.82	0.84	478			1.16	
	29.43	0.79	0.80	477			1.05	
61.9	Mean			476.6	474.6	86079.44	1.60	86081.04

August 14, A. M. PORTSOY.—2d Series.

Clock gaining 42^s.18.

Barometer 30.25 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
60.1	h. m. s. 7.30.40 38.36 46.32 54.28 8. 2.25 10.21 18.18 26.15 34.12 42. 9 50. 7	° 1.31 1.16 1.11 1.05 1.01 0.98 0.94 0.90 0.86 0.83 0.79	° 1.18 1.13 1.08 1.03 0.99 0.96 0.92 0.88 0.84 0.81	476 476 476 477 476 477 477 477 477 477 478			s. 2.28 2.09 1.91 1.74 1.61 1.51 1.39 1.27 1.16 1.08	
60.5	50. 7	0.79	0.81	478				
60.3	Mean			476.7	474.7	86079.51	1.60	86081.11

August 14, P. M.

Barometer 30.27 inches.

62.2	1.15.59 23.53 31.47 39.42 47.37 55.32 2. 3.28 11.23 19.19 27.14 35.11	1.31 1.23 1.18 1.12 1.10 1.05 1.01 0.96 0.91 0.87 0.83	1.27 1.20 1.15 1.11 1.07 1.03 0.98 0.93 0.89 0.85	474 474 475 475 475 476 475 475 476 477			2.64 2.35 2.17 2.02 1.88 1.74 1.57 1.42 1.30 1.19	
62.7	35.11	0.83	0.85	477				
63.4	Mean			475.3	473.2	86078.36	1.83	86080.19

462 Capt. KATER's experiments for determining the variation

August 15, A. M. PORTSOY.—2d Series.

Clock gaining $42^s, 18$.

Barometer 30,25 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
59,9	h. m. s. 7. 16. 40 24. 35 32. 31 40. 26 48. 22 56. 18 8. 4. 14 12. 11 20. 7 28. 4 36. 1	° 1,29 1,23 1,18 1,13 1,08 1,03 0,99 0,96 0,91 0,87 0,83	° 1,26 1,20 1,15 1,10 1,05 1,01 0,97 0,93 0,89 0,85	475 476 475 476 476 476 477 476 477 477 477			s. 2,60 2,36 2,17 1,98 1,81 1,67 1,54 1,42 1,30 1,18	
60,3								
60,1	Mean			476,1	474,1	86079,05	1,80	86080,85

August 15, P. M.

Barometer 30,25 inches.

61,4	1. 11. 5 19. 0 26. 54 34. 50 42. 44 50. 40 58. 36 2. 6. 31 14. 27 22. 23 30. 19	1,21 1,16 1,11 1,07 1,03 0,98 0,94 0,90 0,87 0,84 0,79	1,18 1,13 1,09 1,04 1,00 0,96 0,92 0,88 0,85 0,81	475 474 476 474 476 476 475 476 476 476 476			2,24 2,00 1,95 1,78 1,64 1,51 1,39 1,27 1,14 1,08	
61,6	Mean			475,4	473,4	86078,51	1,62	86080,13

in the length of the pendulum vibrating seconds. 463

August 16, A. M. PORTSOY.—2d Series.

Clock gaining $42^s, 18$.

Barometer 30,18 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
58,0	h. m. s.	°	°				s.	
	7.26.39	1,18	1,15	476			2,16	
	34.35	1,12	1,10	476			1,98	
	42.31	1,08	1,06	477			1,84	
	50.28	1,05	1,02	476			1,70	
	58.24	1,00	0,98	478			1,57	
	8. 6.22	0,96	0,93	477			1,42	
	14.19	0,91	0,89	477			1,30	
	22.16	0,88	0,86	476			1,21	
	30.12	0,85	0,83	478			1,13	
	38.10	0,81	0,79	478			1,02	
58,8	46. 8	0,78						
58,4	Mean			476,9	474,9	86079,66	1,53	86081,19

August 16, P. M.

Barometer 30,17 inches.

60,5	1. 5.32	1,21	1,18	474			2,28	
	13.26	1,15	1,13	476			2,09	
	21.22	1,11	1,08	475			1,91	
	29.17	1,06	1,04	475			1,77	
	37.12	1,02	0,99	476			1,60	
	45. 8	0,97	0,94	475			1,45	
	53. 3	0,91	0,90	476			1,33	
	2. 0.59	0,89	0,87	476			1,24	
	8.55	0,85	0,83	476			1,13	
	16.51	0,82	0,80	477			1,05	
61,3	24.48	0,79						
60,9	Mean			475,6	473,6	86078,67	1,59	86080,26

464 Capt. KATER's experiments for determining the variation

August 17, A. M. PORTSOY.—2d Series.

Clock gaining 4^{rs}.18.

Barometer 30,15 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
59,5	h. m. s.	°	°				s.	
	7.18.34	1,27	1,24	475			2,53	
	26.29	1,22	1,19	475			2,32	
	34.24	1,17	1,14	475			2,13	
	42.19	1,12	1,09	476			1,95	
	50.15	1,07	1,04	476			1,77	
	58.11	1,02	1,00	476			1,64	
	8. 6. 7	0,99	0,97	476			1,54	
	14. 3	0,95	0,93	476			1,42	
	21.59	0,91	0,89	476			1,30	
	29.55	0,88	0,86	477			1,21	
60,2	37.52	0,84						
59,8	Mean			475,8	473,8	86078,82	1,78	86080,60

August 17, P. M.

Barometer 30,16 inches.

61,0	1.28.27	1,30	1,26	474			2,60	
	36.21	1,23	1,20	475			2,36	
	44.16	1,18	1,15	474			2,17	
	52.10	1,13	1,11	474			2,02	
	2. 0. 4	1,09	1,06	476			1,84	
	8. 0	1,04	1,02	475			1,71	
	15.55	1,00	0,97	476			1,54	
	23.51	0,95	0,93	475			1,49	
	31.46	0,92	0,90	476			1,33	
	39.42	0,88	0,86	476			1,21	
61,5	47.38	0,84						
61,2	Mean			475,1	473,1	86078,21	1,84	86080,11

August 18, A. M. PORTSOY.—2d Series.

Clock gaining 42^s.18

Barometer 30.14 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
58.4	h. m. s.	°	°				s.	
	7.32.46	1.20	1.18	475			2.28	
	40.41	1.16	1.13	476			2.09	
	48.37	1.10	1.08	476			1.91	
	56.33	1.06	1.03	476			1.74	
	8. 4.29	1.01	0.99	476			1.60	
	12.25	0.98	0.95	476			1.48	
	20.21	0.93	0.91	477			1.36	
	28.18	0.89	0.87	477			1.24	
	36.15	0.85	0.83	477			1.13	
	44.12	0.81	0.79	477			1.02	
58.5	52. 9	0.78						
58.4	Mean			476.3	474.3	86079.20	1.59	86080.79

August 18, P. M.

Barometer 30.14 inches.

59.7	1. 0.54	1.21	1.18	475			2.28	
	8.49	1.15	1.12	475			2.06	
	16.44	1.10	1.08	475			1.92	
	24.39	1.06	1.03	475			1.74	
	32.34	1.01	0.99	475			1.60	
	40.29	0.97	0.95	476			1.48	
	48.25	0.94	0.91	476			1.36	
	56.21	0.89	0.87	476			1.24	
	2. 4.17	0.85	0.83	476			1.13	
	12.13	0.82	0.80	476			1.03	
60.7	20. 9	0.79						
60.2	Mean			475.5	473.5	86078.59	1.59	86080.18

466 Capt. KATER's experiments for determining the variation

August 19, A. M. PORTSOY.—2d Series.

Clock gaining $42^s, 18$.

Barometer 30,1 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
57.8	h. m. s.	"	"					
	7.38.35	1,20	1,17	475			2,84	
	46.30	1,14	1,12	476			2,06	
	54.26	1,11	1,08	476			1,92	
	8. 2.22	1,05	1,03	476			1,74	
	10.18	1,01	0,99	477			1,60	
	18.15	0,98	0,96	476			1,51	
	26.11	0,94	0,91	477			1,36	
	34. 8	0,89	0,87	477			1,24	
	42. 5	0,85	0,83	477			1,13	
	50. 2	0,81	0,79	477			1,02	
	57.5	58.59	0,78					
57,4	Mean			476,4	474,4	86079,27	1,58	86080,85

August 31, A. M. at LEITH FORT.—1st. Series

Clock gaining 26^s.85 in a mean solar day. Barometer 29.95 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations. in 24 hours.
56 ^o .5	h. m. s.	°	°				s.	
	7.29. 9	1.10	1.08	492			1.91	
	37.21	1.06	1.04	494			1.77	
	45.35	1.02	0.99	494			1.61	
	54.49	0.97	0.95	493			1.48	
	8. 2. 2	0.93	0.90	495			1.33	
	10.17	0.88	0.86	495			1.21	
	18.32	0.85	0.83	494			1.13	
	26.46	0.82	0.80	494			1.05	
	35. 0	0.78	0.76	496			0.94	
	43.16	0.75	0.74	494			0.89	
56.67	51.30	0.73						
56.6	Mean			494.1	492.1	86077.01	1.33	86078.34

August 31, P. M. Barometer 29.85 inches.

58.6	1.33.24	1.17	1.14	492			2.13	
	41.36	1.12	1.09	492			1.95	
	49.48	1.07	1.05	492			1.80	
	58. 0	1.03	1.00	493			1.64	
	2. 6.13	0.98	0.96	493			1.51	
	14.26	0.94	0.91	492			1.36	
	22.38	0.89	0.87	494			1.24	
	30.52	0.86	0.84	493			1.16	
	39. 5	0.83	0.81	493			1.08	
	47.18	0.80	0.78	493			1.00	
58.2	55.31	0.77						
58.9	Mean			492.7	490.7	86076.02	1.48	86077.50

468 Capt. KATER's experiments for determining the variation

September 1, A. M. LEITH FORT.—1st Series.

Clock gaining 26',85.

Barometer 29,55 inches.

Temp.	Time of coincidence	Arc of vibration	Mean Arc.	Interval in seconds.	No of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
58,7	<i>h. m. s.</i> 8. 3. 0 11. 9 19.18 27.28 35.39 43.49 52. 0 9. 0.12 8.22 16.34 24.45	1,22 1,17 1,12 1,07 1,03 0,98 0,94 0,90 0,87 0,83 0,79	1,19 1,14 1,09 1,05 1,00 0,96 0,92 0,88 0,85 0,81	489 489 490 491 490 491 492 490 492 491			2,32 2,13 1,95 1,80 1,64 1,51 1,39 1,27 1,18 1,07	
58,8								
58,7	Mean			490,5	488,5	86074,45	1,63	86076,08

September 1, P. M.

Barometer 29,49 inches.

59,7	1.50.37 58.46 2. 6.37 15. 7 24.16 31.37 39.37 47.48 55.59 3. 4. 9 12.21	1,12 1,07 1,02 0,98 0,93 0,89 0,85 0,82 0,78 0,76 0,73	1,09 1,04 1,00 0,95 0,91 0,87 0,83 0,80 0,77 0,74	489 491 490 489 491 490 491 491 490 492			1,95 1,77 1,64 1,48 1,36 1,24 1,13 1,05 1,97 1,89	
60,5								
60,1	Mean			490,4	488,4	86074,37	1,35	86073,72

September 2, A. M. LEITH FORT.—1st Series.

Clock gaining 26^s.85. Barometer 29.58 inches. .

Temp.	Time of coin- cidence.	Arc of vibra- tion.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correc- tion for Arc.	Vibrations in 24 hours.
58.0	h. m. s.	°	°				s.	
	8.19.41	1.12	1.09	489			1.95	
	27.50	1.07	1.04	489			1.77	
	35.59	1.02	0.99	489			1.61	
	44. 8	0.97	0.95	490			1.48	
	52.18	0.93	0.90	490			1.33	
	9. 0.28	0.88	0.86	489			1.21	
	8.37	0.85	0.83	490			1.13	
	16.47	0.82	0.80	490			1.05	
	24.57	0.78	0.76	490			0.94	
	33. 7	0.75	0.74	490			0.89	
58.7	41.17	0.73						
58.4	Mean			489.6	487.6	86073.80	1.34	86075.14

September 2, P. M. Barometer 29.68 inches.

59.8	1.15.32	1.08	1.05	488			1.80	
	23.40	1.03	1.00	488			1.64	
	31.48	0.98	0.96	489			1.51	
	39.57	0.94	0.92	488			1.39	
	48. 5	0.91	0.89	490			1.30	
	56.15	0.87	0.85	489			1.18	
	2. 4.24	0.83	0.81	489			1.08	
	12.33	0.80	0.76	489			1.00	
	20.42	0.76	0.74	489			0.90	
	28.51	0.73	0.71	490			0.82	
60.0	37. 1	0.70						
59.9	Mean			488.9	486.9	86073.29	1.06	86074.55

470 *Capt. KATER's experiments for determining the variation*

September 3, A. M. LEITH FORT.—1st Series.

Clock gaining 26^s.85.

Barometer 29.95 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
56 ^o .9	h. m. s.						s.	
	7.49.55	1.07	1.05	489			1.80	
	58. 4	1.03	1.00	489			1.64	
	8. 6.13	0.98	0.96	489			1.51	
	14.22	0.94	0.92	490			1.39	
	22.32	0.91	0.89	490			1.30	
	30.42	0.87	0.85	490			1.18	
	38.52	0.83	0.81	490			1.08	
	47. 2	0.79	0.77	491			0.97	
	55.13	0.76	0.74	489			0.90	
	9. 3.22	0.73	0.71	490			0.82	
57.9	11.32	0.70						
57.4	Mean			489.7	487.7	86073.87	1.26	86075.13

September 3. P. M.

Barometer 29.97 inches.

59.5	1. 6. 6	1.18	1.15	486			2.17	
	14.12	1.13	1.09	487			1.95	
	22.19	1.06	1.04	489			1.77	
	30.28	1.03	1.00	487			1.64	
	38.38	0.98	0.96	488			1.51	
	46.43	0.94	0.92	489			1.39	
	54.52	0.90	0.88	488			1.27	
	2. 3. 0	0.86	0.84	488			1.15	
	11. 8	0.83	0.81	489			1.08	
	19.17	0.79	0.77	489			0.97	
	27.26	0.76						
59.9								
59.7	Mean			488	486	86072.64	1.49	86074.13

in the length of the pendulum vibrating seconds.

471

September 4, A. M. LEITH FORT.—1st. Series.

Clock gaining 26".85.

Barometer 29.78 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
59.3	h. m. s.	°	°				s.	
	7.55.48	1.18	1.15	486			2.17	
	8. 3.54	1.13	1.10	488			1.98	
	12. 2	1.08	1.05	488			1.80	
	20.10	1.02	1.00	488			1.64	
	28.18	0.98	0.95	488			1.48	
	36.26	0.93	0.91	488			1.36	
	44.34	0.90	0.88	488			1.27	
	52.42	0.87	0.85	489			1.18	
	9. 0.51	0.83	0.81	488			1.08	
	8.59	0.79	0.77	489			0.97	
59.8	17. 8	0.76						
59.5	Mean			488	486	86072.64	1.49	86074.13

September 4, P. M.

Barometer 29.76 inches.

61.6	1.31.40	1.17	1.14	486			2.13	
	29.46	1.12	1.09	486			1.95	
	37.52	1.06	1.04	486			1.77	
	45.58	1.02	1.00	487			1.64	
	54. 5	0.98	0.95	487			1.48	
	2. 2.12	0.93	0.91	486			1.36	
	10.18	0.90	0.88	488			1.27	
	18.26	0.87	0.85	487			1.18	
	26.33	0.83	0.81	487			1.08	
	34.40	0.79	0.77	487			0.97	
62.2	42.47	0.76						
61.9	Mean			486.7	484.7	86071.68	1.48	86073.16

472 Capt. KATER's experiments for determining the variation

September 5, A. M. LEITH FORT.—1st Series.

Clock gaining 26^s.85.

Barometer 29.85 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
60.0	h. m. s.		°				°.	
	7.44. 8	1.19	1.16	484			2.21	
	52.12	1.14	1.11	486			2.02	
	8. 0.18	1.09	1.06	485			1.84	
	8.23	1.04	1.02	485			1.71	
	16.28	1.00	0.98	486			1.57	
	24.34	0.96	0.94	486			1.45	
	32.40	0.93	0.91	486			1.36	
	40.46	0.89	0.86	486			1.21	
	48.52	0.84	0.82	486			1.10	
	56.58	0.81	0.80	486			1.05	
60.6	9. 5. 4	0.79						
60.8	Mean			485.6	483.6	86070.88	1.55	86072.43

September 5, P. M.

Barometer 29.83 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
62.0	0.58.30	1.14	1.11	485			2.02	
	1. 0.35	1.09	1.07	485			1.88	
	14.38	1.05	1.02	485			1.71	
	22.45	0.99	0.97	484			1.54	
	30.47	0.95	0.93	485			1.42	
	38.52	0.92	0.90	484			1.32	
	46.56	0.88	0.87	485			1.24	
	55. 1	0.86	0.83	485			1.13	
	2. 5. 6	0.81	0.79	484			1.02	
	11.10	0.78	0.76	486			0.94	
62.3	19.16	0.75						
62.1	Mean			484.6	483.6	86070.15	1.42	86071.57

September 6, A. M. LEITH FORT.—1st Series.

Clock gaining 26^s.85.

Barometer 29.6 inches.

Tem	Time of coincidence	Arc of vibration.	Mean Arc.	Interval in seconds	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
ⁿ 59,7	h. m. s.	°	•				s.	
	7.49.27	1,13	1,10	483			1,98	
	57.30	1,08	1,05	483			1,80	
	8. 5.33	1,03	1,00	483			1,64	
	13.36	0,98	0,96	484			1,51	
	21.40	0,94	0,92	493			1,38	
	29.43	0,90	0,88	483			1,27	
	37.46	0,86	0,84	484			1,15	
	45.50	0,83	0,81	484			1,07	
	53.54	0,79	0,77	485			0,97	
	9. 1.59	0,75	0,73	485			0,87	
60,2	10. 4	0,72						
59,9	Mean			483,7	481,7	86069,49	1,36	86070,85

September 6, P. M.

Barometer 29.62 inches.

61,4	1.11.44	1,18	1,15	481			2,17	
	19.45	1,13	1,10	482			1,98	
	27.47	1,08	1,05	483			1,80	
	35.50	1,03	1,00	482			1,64	
	43.52	0,98	0,96	484			1,51	
	51.56	0,95	0,93	483			1,42	
	59.59	0,91	0,89	483			1,30	
	2. 8. 2	0,87	0,85	483			1,18	
	16. 5	0,83	0,81	484			1,07	
	24. 9	0,81	0,78	483			1,00	
61,3	32.12	0,78						
61,4	Mean			482,8	480,8	86068,82	1,51	86070,33

September 9, A. M. LEITH FORT.—2d Series.

Clock gaining $34^s.1$ in a mean solar day. Bar. 29.9 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correc- tion for Arc.	Vibrations in 24 hours.
54.2	h. m. s.	°	°				s.	
	7.55.58	1.16	1.14	481			2.13	
	8. 3.59	1.12	1.10	482			1.98	
	12. 1	1.08	1.06	482			1.81	
	20. 3	1.04	1.02	482			1.70	
	28. 5	1.01	0.99	482			1.60	
	36. 7	0.97	0.95	483			1.48	
	44.10	0.93	0.91	482			1.36	
	52.12	0.90	0.89	483			1.30	
	9. 0.15	0.88	0.86	482			1.21	
	8.17	0.84	0.82	482			1.10	
54.3	16.19	0.81						
54.3	Mean			482.1	480.1	86075.53	1.57	86077.10

September 9, P. M.

Barometer 29.95 inches.

55.5	1.13.37	1.19	1.16	481			2.21	
	21.38	1.14	1.12	480			2.06	
	29.38	1.10	1.06	482			1.84	
	37.40	1.03	1.01	481			1.67	
	45.41	1.00	0.98	481			1.57	
	53.42	0.96	0.94	482			1.45	
	2. 1.44	0.92	0.90	481			1.33	
	9.45	0.88	0.86	482			1.21	
	17.47	0.85	0.83	482			1.13	
	25.49	0.82	0.81	483			1.04	
55.7	33.52	0.79						
55.6	Mean			481.5	479.5	86075.07	1.56	86076.63

September 10, A. M. LEITH FORT.—2d Series.

Clock gaining, 34', 10.

Barometer 29.94 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correc- tion for Arc.	Vibrations in 24 hours.
52,1	h. m. s. 7.59.12 8. 7.14 15.16 23.19 31.21 39.24 47.27 55.30 9. 3.33 11.36 19.39	° 1,15 1,10 1,06 1,02 0,98 0,94 0,90 0,87 0,84 0,80 0,77	° 1,12 1,08 1,04 1,00 0,96 0,92 0,88 0,85 0,82 0,77	° 482 482 483 482 483 483 483 483 483 483 483			s. 2,06 1,91 1,78 1,64 1,51 1,39 1,27 1,18 1,11 0,97	
52,8	Mean			482,7	480,7	86075,97	1,48	86077,45

September 10, P. M.

Barometer 29.91 inches.

54,2	1. 8.54 16.56 24.57 32.59 41. 1 49. 3 57. 4 9. 5. 6 13. 9 21.12 29.14	1,14 1,10 1,04 1,00 0,96 0,93 0,89 0,84 0,81 0,78 0,74	1,12 1,07 1,02 0,98 0,94 0,91 0,86 0,82 0,79 0,76	482 481 482 482 482 481 482 483 483 483 482			2,06 1,88 1,71 1,57 1,45 1,36 1,21 1,10 1,02 0,94	
54,3	Mean			482	480	86075,45	1,53	86076,98

476 *Capt. KATER's experiments for determining the variation*

September 11, A. M. LEITH FORT.—2d Series.

Clock gaining 34', 10.

Barometer 29.92 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
51.3	h. m. s.	°	°				°	
	7.58.14	1.17	1.15	481			2.16	
	8. 6.15	1.13	1.10	482			1.38	
	14.17	1.08	1.06	482			1.84	
	22.19	1.05	1.03	482			1.74	
	30.21	1.01	0.99	483			1.60	
	38.24	0.98	0.96	482			1.51	
	46.26	0.94	0.91	482			1.36	
	54.28	0.89	0.87	483			1.34	
	9. 2.31	0.86	0.84	482			1.16	
	10.33	0.82	0.80	483			1.05	
51.8	18.36	0.79						
51.5	Mean			482.2	480.2	86075.60	1.56	86077.16

September 11, P. M.

Barometer 29.95 inches.

53.2	1.12. 9	1.14	1.11	481			2.02	
	20.10	1.09	1.06	481			1.84	
	28.11	1.04	1.01	482			1.67	
	36.13	0.99	0.97	481			1.54	
	44.14	0.93	0.93	482			1.42	
	52.16	0.91	0.89	482			1.30	
	2. 0.18	0.88	0.86	483			1.21	
	8.21	0.84	0.82	482			1.10	
	16.23	0.81	0.79	482			1.02	
	24.25	0.78	0.76	482			0.94	
53.5	32.27	0.74						
53.8	Mean			481.8	479.8	86075.30	1.41	86076.71

in the length of the pendulum vibrating seconds. 477

September 12, A. M. LEITH FORT.—2d. Series.

Clock gaining 34',10.

Barometer 30,14 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
52,8	h. m. s.	°	°				s.	
	8.16.19	1,17	1,14	481			2,13	
	24.20	1,12	1,09	481			1,95	
	32.21	1,07	1,04	480			1,77	
	40.21	1,02	1,00	483			1,64	
	48.23	0,98	0,96	481			1,51	
	56.24	0,94	0,92	482			1,38	
	9. 4.26	0,91	0,89	482			1,30	
	12.28	0,87	0,85	482			1,18	
	20.30	0,83	0,81	483			1,08	
	28.33	0,80	0,78	481			1,00	
53,4	36.34	0,77						
53,1	Mean			481,6	479,6	86075,15	1,49	86076,64

September 12, P. M.

Barometer 30,14 inches.

53,9	0. 8.28	1,16	1,13	480			2,09	
	16.28	1,11	1,08	481			1,92	
	24.29	1,06	1,03	480			1,74	
	32.29	1,01	0,99	481			1,60	
	40.30	0,97	0,95	481			1,48	
	48.31	0,93	0,90	482			1,38	
	56.33	0,88	0,86	481			1,21	
	1. 4.34	0,85	0,83	481			1,13	
	12.35	0,82	0,80	482			1,05	
	20.37	0,79	0,77	482			0,97	
54,5	28.39	0,76						
54,2	Mean			481,1	479,1	86074,77	1,45	86076,22

September 13, A. M. LEITH FORT.—2d. Series.

Clock gaining 34'.10.

Barometer 30.28 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
53.6	h. m. s.	°	°				s.	
	8.37.37	1.14	1.11	479			2.02	
	45.36	1.09	1.06	480			1.84	
	53.36	1.04	1.02	481			1.71	
	9. 1.37	1.00	0.98	481			1.57	
	9.38	0.96	0.94	481			1.45	
	17.39	0.92	0.90	481			1.33	
	25.40	0.88	0.86	481			1.21	
	33.41	0.84	0.82	481			1.10	
	41.42	0.81	0.79	481			1.02	
	49.42	0.78	0.76	483			0.94	
54.4	57.45	0.75						
54.0	Mean			480.9	478.9	86074.63	1.42	86076.05

September 13, P. M.

Barometer 30.24 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
55.0	h. m. s.	°	°				s.	
	1. 7.18	1.12	1.09	479			1.95	
	15.17	1.07	1.04	479			1.77	
	23.16	1.02	1.00	480			1.64	
	31.16	0.98	0.96	480			1.51	
	39.16	0.94	0.93	481			1.39	
	47.17	0.91	0.89	480			1.30	
	55.17	0.87	0.85	480			1.18	
	2. 3.17	0.83	0.81	481			1.06	
	11.18	0.80	0.78	480			1.00	
	19.18	0.77	0.75	481			0.92	
56.3	27.19	0.74						
55.9	Mean			480.1	478.1	86074.03	1.37	86075.40

September 14, A. M. LEITH FORT.—2nd Series.

Clock gaining 34', 10. Barometer 29,89 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				s.	
56,2	8.16.19	1,09	1,06	480			2,13	
	24.19	1,04	1,02	479			1,95	
	32.18	1,00	0,98	480			1,80	
	40.18	0,97	0,95	480			1,64	
	48.18	0,93	0,91	480			1,51	
	56.18	0,89	0,87	480			1,38	
	9. 4.18	0,85	0,83	490			1,27	
	12.18	0,82	0,80	481			1,18	
	20.19	0,78	0,77	480			1,10	
	28.19	0,76	0,74	481			0,99	
56,6	36.20	0,73						
56,4	Mean			480,1	478,1	86074,03	1,32	86075,35

September 14, P. M.

Barometer 29,85 inches.

57,1	1.21.53	1,16	1,14	478			2,13	
	29.51	1,12	1,09	479			1,95	
	37.50	1,07	1,05	478			1,80	
	45.48	1,03	1,00	480			1,64	
	53.48	0,98	0,96	479			1,51	
	9. 1.47	0,94	0,92	479			1,38	
	9.46	0,90	0,88	479			1,27	
	17.45	0,87	0,85	480			1,18	
	25.45	0,84	0,82	480			1,10	
	33.45	0,80	0,78	480			0,99	
57,2	41.45	0,77						
57,1	Mean			479,2	477,2	86073,36	1,50	86074,86

480 Capt. KATER's experiments for determining the variation

October 3, A. M. at CLIFTON.

Clock losing 10', 60 in a mean solar day. Barometer 29,22 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
57,2	h. m. s. 7.32.46 41.34 50.23 59.10 8. 7.59 16.48 25.38 34.27 43.17 52. 7 57.7	° 1,23 1,17 1,12 1,07 1,02 0,98 0,93 0,88 0,84 0,82 0,78	° 1,20 1,14 1,09 1,04 1,00 0,95 0,90 0,86 0,83 0,80	528 529 527 529 529 530 529 530 530 530 531			2,36 2,13 1,95 1,77 1,64 1,48 1,33 1,21 1,13 1,05	
57,4	Mean			529,2	527,2	86062,91	1,61	86064,52

October 3, P. M.

Barometer, 29,20 inches.

58,2	1.45.23 2. 5. 9 13.57 22.44 31.31 40.18 49. 7 57.56 3. 6.48 15.34 24.23	1,98 1,83 1,17 1,12 1,07 1,02 0,97 0,93 0,89 0,84 0,81	1,25 1,20 1,14 1,09 1,04 0,99 0,95 0,91 0,86 0,82	526 528 527 527 528 528 529 529 529 529 529			2,57 2,36 2,13 1,95 1,77 1,61 1,48 1,33 1,21 1,10	
58,2	Mean			528	526	86062,17	1,75	86063,92

October 4, A. M. CLIFTON.

Clock losing 10', 60.

Barometer 29,18 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
57,1	h. m. s.	°	°				s.	
	9.44. 8	1,23	1,20	529			2,38	
	52.57	1,17	1,15	528			2,17	
	10. 1.45	1,12	1,09	528			1,95	
	10.33	1,06	1,04	529			1,77	
	19.22	1,02	0,99	529			1,61	
	28.11	0,97	0,94	530			1,45	
	37. 1	0,92	0,90	528			1,33	
	45.49	0,88	0,85	529			1,18	
	54.38	0,83	0,81	531			1,08	
	11. 3.29	0,79	0,77	530			0,97	
57,3	12.19	0,76						
57,2	Mean			529,1	527,1	86062,85	1,59	86064,44

October 4, P. M.

Barometer 29,13 inches.

57,2	1. 6.12	1,24	1,22	528			2,43	
	15. 0	1,20	1,16	527			2,20	
	24.47	1,13	1,10	528			1,98	
	32.35	1,08	1,06	529			1,84	
	41.24	1,04	1,01	528			1,67	
	50.12	0,99	0,96	527			1,51	
	58.59	0,94	0,91	529			1,36	
	2. 7.48	0,89	0,87	529			1,24	
	16.37	0,85	0,83	530			1,13	
	25.27	0,82	0,80	531			1,05	
57,3	34.18	0,78						
57,2	Mean			528,6	526,6	86062,54	1,64	86064,18

482 Capt. KATER's experiments for determining the variation

October 5, A. M. CLIFTON.

Clock losing 10', 60.

Barometer 29,10 inches.

Temp.	Time of coin- cidence.	Arc of vibra- tion.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correc- tion to Arc	Vibrations in 24 hours.
54,8	h. m. s.	°	°				s.	
	7.57. 0	1,23	1,21	529			2,40	
	8. 6.55	1,18	1,15	530			2,17	
	14.45	1,12	1,09	530			1,95	
	23.35	1,07	1,04	529			1,77	
	32.21	1,02	0,99	531			1,61	
	41.15	0,97	0,95	531			1,48	
	50. 6	0,93	0,91	530			1,36	
	58.56	0,89	0,86	531			1,21	
	9. 7.47	0,84	0,82	531			1,10	
	16.38	0,81	0,79	532			1,02	
55,4	25.30	0,78						
55,1	Mean			530,4	528,4	86063,65	1,61	86065,26

October 5, P. M.

Barometer 29,08 inches.

55,6	1.52.53	1,37	1,34	528			2,52	
	9. 1.41	1,32	1,30	529			2,30	
	10.30	1,18	1,15	529			2,17	
	19.19	1,13	1,10	529			1,98	
	28. 8	1,07	1,04	529			1,77	
	36.57	1,02	1,00	530			1,64	
	45.47	0,98	0,95	531			1,48	
	54.38	0,93	0,91	530			1,36	
	3. 3.28	0,89	0,87	529			1,24	
	12.17	0,86	0,84	528			1,15	
55,9	21. 9	0,82						
55,7	Mean			529,6	527,6	86063,16	1,77	86064,93

October 6, A. M. CLIFTON.

Clock losing 10,60

Barometer 29,01 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
53,2	h. m. s.	°	°				s.	
	7.52.51	1,31	1,28	529			2,68	
	8. 1.43	1,25	1,22	530			2,44	
	10.33	1,19	1,16	531			2,20	
	19.24	1,13	1,10	530			1,98	
	28.14	1,08	1,05	531			1,80	
	37. 5	1,03	1,00	531			1,64	
	45.56	0,98	0,95	531			1,48	
	54.47	0,93	0,90	531			1,33	
	9. 3.38	0,88	0,86	532			1,21	
	12.30	0,85	0,83	533			1,13	
53,6	21.23	0,82						
53,4	Mean			530,9	528,9	86063,96	1,79	86065,75

October 6, P. M.

Barometer 29,10 inches.

53,9	1.55. 5	1,22	1,19	529			2,32	
	2. 3.54	1,16	1,13	531			2,09	
	12.45	1,11	1,08	529			1,91	
	21.34	1,06	1,03	530			1,74	
	30.21	1,01	0,99	530			1,60	
	39.14	0,97	0,94	531			1,45	
	48. 5	0,92	0,89	531			1,30	
	56.56	0,87	0,85	530			1,15	
	3. 5.46	0,83	0,81	530			1,08	
	14.36	0,79	0,77	531			0,97	
55,1	23.27	0,76						
54,5	Mean			530,2	528,2	86063,52	1,56	86065,08

484 *Capt. KATER's experiments for determining the variation*

October 7, A. M. CLIFTON.

Clock losing 10^s.60.

Barometer 29.30 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
52.5	h. m. s.	°	"				"	
	9.34.54	1.28	1.25	529			2.57	
	43.43	1.23	1.20	530			2.36	
	52.33	1.18	1.15	529			2.17	
	10. 1.22	1.13	1.10	530			1.98	
	10.12	1.07	1.04	531			1.77	
	19. 3	1.02	0.99	531			1.61	
	27.54	0.97	0.94	531			1.45	
	36.45	0.92	0.90	531			1.33	
	45.36	0.88	0.86	531			1.21	
	54.27	0.84	0.82	532			1.10	
53.3	11. 3.19	0.81						
52.9	Mean			530.5	528.5	86063.71	1.76	86065.47

October 7, P. M.

Barometer 29.33 inches.

53.4	1.53.19	1.23	1.20	530			2.36	
	2. 2. 9	1.17	1.14	529			2.13	
	10.58	1.12	1.09	530			1.95	
	19.46	1.07	1.04	530			1.77	
	28.38	1.02	0.99	531			1.60	
	37.29	0.97	0.95	530			1.48	
	46.19	0.93	0.91	531			1.36	
	55.10	0.89	0.86	531			1.21	
	3. 4. 1	0.84	0.83	530			1.13	
	12.51	0.82	0.80	532			1.05	
54.1	21.43	0.78						
53.7	Mean			530.4	528.4	86063.65	1.60	86065.25

October 8, A. M. CLIFTON.

Clock losing 10'.60.

Barometer 29.52 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
51.9	h. m. s.	°	°				°.	
	7.54.51	1.24	1.21	529			2.40	
	8. 3.40	1.19	1.16	530			2.20	
	12.30	1.13	1.10	529			1.98	
	21.19	1.08	1.05	529			1.81	
	30. 8	1.03	1.01	532			1.67	
	39. 0	0.99	0.96	531			1.51	
	47.51	0.93	0.91	531			1.36	
	56.42	0.90	0.88	531			1.27	
	9. 5.53	0.87	0.85	531			1.18	
	14.24	0.83	0.81	532			1.07	
52.5	23.16	0.79						
52.2	Mean			530.5	528.5	86063.71	1.65	86065.36

October 8, P. M.

Barometer 29.57 inches.

2.21. 0	1.23	1.20	529			2.36	
42.49	1.18	1.15	529			2.17	
5. 38	1.13	1.10	529			1.98	
3. 27	1.07	1.04	530			1.77	
17	1.02	1.00	530			1.64	
18. 7	0.98	0.95	531			1.48	
1.58	0.93	0.91	530			1.36	
1.48	0.89	0.87	531			1.24	
1.39	0.85	0.83	531			1.13	
1.30	0.81	0.79	531			1.02	
53.2	1.21	0.78					
52.9	Mean		530.1	528.1	86063.46	1.62	86065.08

October 21, P. M.—at ARBURY HILL.

Clock losing 6^s.2 in a mean solar day. Barometer 29.65 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correc- tion for Arc.	Vibrations in 24 hours.
°	h. m. s.	n	s				s.	
56.7	1.33.46	1.10	1.16	512			2.30	
	48.16	1.13	1.11	514			2.04	
	50.52	1.00	1.07	514			1.88	
	59.26	1.05	1.02	514			1.70	
	2. 8. 0	1.00	0.98	511			1.57	
	16.34	0.96	0.94	514			1.45	
	25. 8	0.92	0.90	515			1.33	
	33.43	0.88	0.86	514			1.21	
	42.17	0.84	0.82	515			1.10	
	50.52	0.81	0.79	515			1.02	
56.7	59.27	0.78						
56.7	Mean			514.1	518.1	86057.70	1.55	86059.25

October 22, A. M. ARBURY HILL.

Clock losing 6.2.

Barometer 29.52 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours
54.1	h. m. s.	°	°				s.	
	9.10.38	1.13	1.12	515			2.06	
	19.13	1.10	1.08	516			1.91	
	27.49	1.06	1.04	516			1.77	
	36.25	1.02	0.99	516			1.61	
	45. 1	0.97	0.95	516			1.48	
	53.37	0.93	0.91	516			1.36	
	10. 2.13	0.89	0.87	517			1.24	
	10.50	0.85	0.83	517			1.13	
	19.27	0.82	0.80	517			1.05	
54.4	28. 4	0.78	0.76	518			0.94	
54.2	Mean			516.4	514.4	86059.20	1.46	86060.66

October 22, P. M.

Barometer 29.50 inches.

54.4	1.52.46	1.14	1.12	516			2.06	
	2. 1.22	1.10	1.08	515			1.91	
	9.57	1.06	1.04	516			1.77	
	18.33	1.02	0.99	517			1.61	
	27.10	0.97	0.94	515			1.45	
	35.45	0.92	0.90	517			1.33	
	44.22	0.88	0.87	516			1.24	
	52.58	0.86	0.84	517			1.13	
	3. 1.35	0.82	0.80	517			1.05	
	10.12	0.78	0.76	516			1.04	
54.4	18.48	0.75						
54.4	Mean			516.2	514.2	86059.07	1.45	86060.52

488 Capt. KATER's experiments for determining the variation

October 23. ARBURY HILL.

Clock losing 6',20.

Barometer 29,50 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
52,6	h. m. s.	°	°				8.	
	9. 8.44	1,21	1,19	516			2,32	
	17.30	1,17	1,14	516			2,13	
	25.56	1,12	1,09	516			1,95	
	34.32	1,06	1,04	516			1,77	
	43. 8	1,01	0,99	517			1,61	
	51.45	0,97	0,95	517			1,48	
	10. 0.22	0,93	0,91	518			1,36	
	9. 0	0,89	0,87	518			1,24	
	17.38	0,86	0,84	516			1,16	
	26.14	0,82	0,81	518			1,08	
53,1	34.52	0,80						
52,8	Mean			516,8	514,8	86059,46	1,61	86061,07

October 23, P. M.

Barometer 29,52 inches.

53,2	1.43.31	1,14	1,12	516			2,06	
	51.57	1,11	1,08	516			1,91	
	3. 0.33	1,06	1,03	516			1,74	
	9. 9	1,02	1,00	516			1,61	
	17.45	0,98	0,96	516			1,48	
	26.21	0,94	0,92	517			1,39	
	34.58	0,91	0,89	517			1,30	
	43.35	0,87	0,85	517			1,18	
	52.12	0,83	0,81	518			1,07	
	3. 0.50	0,80	0,78	518			1,00	
53,2	9.28	0,77						
53,3	Mean			516,7	514,7	86059,40	1,48	86060,88

in the length of the pendulum vibrating seconds. 489

October 24, A. M. ARBURY HILL.

Clock losing 6',20. Barometer 29,57 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				s.	
50,7	9. 9.14	1,17	1,14	517			2,13	
	17.51	1,12	1,09	517			1,95	
	26.28	1,07	1,05	517			1,80	
	35. 5	1,03	1,00	517			1,64	
	43.42	0,98	0,96	517			1,51	
	52.19	0,94	0,92	518			1,39	
	10. 0.57	0,90	0,88	518			1,27	
	9.35	0,86	0,84	517			1,16	
	18.12	0,82	0,80	519			1,05	
	27.51	0,78	0,76	518			0,94	
51,0	35.29	0,75						
50,8	Mean			517,5	515,5	86059,92	1,48	86061,40

October 24, P. M. Barometer 29,55 inches.

50,5	1.35. 5	1,22	1,19	516			2,32	
	43.41	1,17	1,14	516			2,13	
	52.17	1,12	1,10	517			1,98	
	2. 0.54	1,08	1,05	516			1,80	
	9.30	1,03	1,00	517			1,64	
	18. 7	0,98	0,96	517			1,51	
	26.44	0,94	0,92	518			1,39	
	35.22	0,90	0,88	518			1,27	
	44. 0	0,87	0,85	518			1,16	
	52.38	0,83	0,81	518			1,07	
50,8	3. 1. 16	0,79						
50,6	Mean			517,1	515,1	86059,65	1,63	86061,28

490 Capt. KATER's experiments for determining the variation

October 25, A. M. ARBURY HILL.

Clock losing 6', 20.

Barometer 29,56 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
50,7	h. m. s.	"	"				s.	
	9.19.37	1,18	1,15	516			2,17	
	28.13	1,12	1,10	517			1,98	
	36.50	1,08	1,05	516			1,81	
	45.26	1,03	1,00	517			1,64	
	54. 3	0,98	1,95	519			1,48	
	10. 2.42	0,93	0,91	517			1,36	
	11.19	0,89	0,86	517			1,21	
	19.56	0,84	0,83	518			1,13	
	28.34	0,82	0,80	520			1,05	
	37.14	0,79	0,77	518			0,97	
51,2	45.52	0,76						
50,9	Mean			517,5	515,5	86059,92	1,48	86061,40

October 25, P. M.

Barometer 29,54 inches.

52,0	1.45.36	1,14	1,11	516			2,02	
	54.12	1,09	1,06	517			1,84	
	2. 2.49	1,03	1,01	516			1,67	
	11.25	1,00	0,98	517			1,57	
	20. 2	0,96	0,94	517			1,45	
	28.39	0,92	0,90	517			1,33	
	37.16	0,88	0,85	517			1,18	
	45.53	0,83	0,81	518			1,07	
	54.31	0,80	0,78	517			1,00	
	3. 3. 8	0,77	0,75	518			0,92	
52,6	11.46	0,73						
52,3	Mean			517	515	86059,59	1,41	86061,00

in the length of the pendulum vibrating seconds. 491

October 26, A. M. ARBURY HILL.

Clock losing 6^s.20.

Barometer 29.55 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				s.	
51.9	9. 7.12	1.16	1.13	516			2.10	
	15.48	1.11	1.08	516			1.91	
	24.24	1.06	1.03	516			1.74	
	33. 0	1.01	0.98	516			1.57	
	41.36	0.96	0.94	516			1.45	
	50.12	0.92	0.90	517			1.33	
	58.49	0.88	0.85	517			1.18	
	10. 7.26	0.83	0.82	516			1.10	
	16. 2	0.81	0.79	518			1.02	
	24.40	0.78	0.75	516			0.92	
52.5	33.16	0.73						
52.2	Mean			516.4	514.4	86059.20	1.43	86060.63

October 26, P. M.

Barometer 29.55 inches.

53.5	2. 8.18	1.15	1.13	515			2.10	
	16.53	1.11	1.08	515			1.91	
	25.28	1.06	1.03	515			1.74	
	34. 3	1.01	0.98	515			1.57	
	42.38	0.96	0.94	516			1.45	
	51.14	0.92	0.90	516			1.33	
	59.50	0.88	0.86	516			1.21	
	3. 8.26	0.85	0.83	516			1.13	
	17. 2	0.81	0.79	516			1.02	
	25.38	0.77	0.75	516			0.92	
53.9	34.14	0.73						
53.7	Mean			515.6	513.6	86058.68	1.44	86060.12

492 Capt. KATER's experiments for determining the variation

1819, March 8, A. M. at Mr. Browne's house, LONDON.

Clock gaining 1^s.75 in a mean solar day. Barometer 30,10 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
49,8	h. m. s.	"	"				"	
	10.40. 1	1,18	1,15	502			2,16	
	48.23	1,12	1,10	503			1,99	
	56.46	1,08	1,05	503			1,81	
	11. 5. 9	1,03	1,00	503			1,64	
	13.32	0,98	0,96	504			1,51	
	21.56	0,94	0,92	504			1,39	
	30.20	0,91	0,89	504			1,30	
	38.44	0,87	0,85	504			1,19	
	47. 8	0,83	0,81	504			1,07	
	55.32	0,80	0,78	505			1,00	
50,3	0. 3.57	0,77						
50,0	Mean			503,6	501,6	86058,61	1,51	86060,12

March 9, A. M. LONDON.

Clock gaining 1^s.85.

Barometer 30,10 inches.

49,8	10.35.36	1,14	1,11	503			2,02	
	43.59	1,09	1,07	503			1,88	
	52.22	1,05	1,03	503			1,74	
	11. 0.45	1,01	0,98	504			1,57	
	9. 9	0,96	0,94	504			1,45	
	17.34	0,92	0,90	503			1,33	
	25.57	0,88	0,86	504			1,21	
	34.21	0,84	0,82	504			1,11	
	42.45	0,81	0,79	504			1,02	
	51. 9	0,78	0,76	504			0,95	
50,5	59.33	0,74						
50,1	Mean			503,7	501,7	86058,78	1,43	86060,21

in the length of the pendulum vibrating seconds. 493

March 15, A. M. LONDON.

Clock gaining 2^s.24.

Barometer 30.14 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				s.	
51.5	10.24.21	1.14	1.11	501			2.02	
	32.42	1.08	1.06	501			1.84	
	41.3	1.04	1.01	501			1.67	
	49.24	0.99	0.96	503			1.51	
	57.47	0.94	0.92	502			1.38	
	11. 6.9	0.91	0.89	502			1.30	
	14.31	0.88	0.86	502			1.21	
	22.53	0.84	0.82	503			1.10	
	31.16	0.81	0.79	502			1.02	
	39.38	0.78	0.76	503			0.94	
52.2	48.1	0.74						
51.8	Mean			502	500	86058.01	1.40	86059.41

March 16, A. M. LONDON.

Clock gaining 2^s.24.

Barometer 30.0 inches.

	10.20.42	1.18	1.15	500			2.17	
	29. 2	1.12	1.09	501			1.95	
	37.23	1.07	1.05	501			1.80	
	45.44	1.03	1.00	501			1.64	
	54. 5	0.98	0.96	501			1.51	
	11. 2.26	0.95	0.93	502			1.42	
	10.48	0.92	0.90	501			1.33	
	19. 9	0.88	0.86	501			1.21	
	27.30	0.84	0.82	502			1.10	
	35.52	0.81	0.79	502			1.02	
53.1	44.14	0.78						
52.7	Mean			501.2	499.2	86057.46	1.52	86058.98

494 Capt. KATER's experiments for determining the variation

March 17, A. M. LONDON.

Clock gaining 2^s.24.

Barometer 30.10 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
53.2	h. m. s.	°	°				s.	
	10.24.19	1.17	1.15	500			2.17	
	32.39	1.13	1.10	501			1.98	
	41. 0	1.08	1.06	500			1.84	
	49.20	1.04	1.02	501			1.71	
	57.41	1.00	0.97	501			1.54	
	11. 6. 2	0.95	0.93	502			1.42	
	14.24	0.92	0.90	502			1.33	
	22.46	0.88	0.86	500			1.21	
	31. 6	0.84	0.82	502			1.10	
	39.28	0.81	0.79	502			1.02	
53.7	47.50	0.78						
53.5	Mean			501.1	499.1	86057.39	1.53	86058.92

March 18, A. M. LONDON.

Clock gaining 2^s.24.

Barometer 30.21 inches.

53.5	10.39.58	1.16	1.13	501			2.09	
	48.19	1.11	1.08	500			1.91	
	56.39	1.06	1.04	501			1.77	
	11. 5. 0	1.02	0.99	501			1.60	
	13.21	0.97	0.95	501			1.48	
	21.42	0.93	0.91	501			1.36	
	30. 3	0.90	0.88	501			1.27	
	38.24	0.86	0.84	502			1.15	
	46.46	0.82	0.80	502			1.06	
	55. 8	0.78	0.77	502			0.97	
53.2	0. 2.30	0.76						
53.2	Mean			501.2	499.2	86057.46	1.47	86058.93

May 11, A. M. at SHANKLIN FARM.

Clock losing 9^s.4 in a mean solar day. Barometer 30.17 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
60.5	h. m. s. 9.27. 9	1.32	1.18	506			s. 2.38	
	35.35	1.14	1.11	508			2.02	
	44. 3	1.09	1.06	508			1.84	
	52.31	1.04	1.01	508			1.68	
	10. 0.59	0.99	0.97	508			1.54	
	9.27	0.95	0.93	509			1.42	
	17.56	0.92	0.90	509			1.33	
	26.25	0.88	0.85	508			1.18	
	34.53	0.83	0.81	509			1.07	
	43.22	0.79	0.77	509			0.97	
61.3	51.51	0.76						
60.9	Mean			508.2	506.2	86050.61	1.53	86052.14

May 11, P. M.

Barometer 30.16 inches.

61.6	0.22.53	1.20	1.17	506			2.24	
	31.19	1.14	1.11	507			2.02	
	39.46	1.08	1.06	508			1.84	
	48.14	1.04	1.01	507			1.68	
	56.41	0.99	0.96	507			1.51	
	1. 5. 8	0.94	0.92	508			1.39	
	13.36	0.91	0.89	507			1.30	
	22. 3	0.87	0.85	508			1.18	
	30.31	0.83	0.81	508			1.07	
	38.59	0.80	0.78	509			1.00	
62.1	47.28	0.76						
61.8	Mean			507.5	505.5	86050.21	1.62	86051.73

496 Capt. KATER's experiments for determining the variation

May 12, A. M. SHANKLIN FARM.

Clock losing 9^s.4.

Barometer 30,10 inches.

Temp.	Time of coincidence.	Arc of vibrations.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
60,6	h. m. s.	°	°				6.	
	9.17.32	1,18	1,15	507			2,17	
	25.59	1,13	1,10	508			1,98	
	34.27	1,08	1,05	507			1,81	
	42.54	1,03	1,00	507			1,64	
	51.21	0,98	0,96	508			1,51	
	59.49	0,91	0,92	508			1,39	
	10. 8.17	0,90	0,88	509			1,27	
	16.46	0,86	0,84	509			1,16	
	25.15	0,83	0,81	508			1,08	
	33.43	0,79	0,77	508			0,97	
	42.11	0,76						
61,0	Mean			507,9	505,9	86050,46	1,50	86051,96

May 12, P. M.

Barometer 30,09 inc

61,2	0.16.43	1,21	1,18	507			2,28	
	25.10	1,16	1,13	507			2,09	
	33.37	1,11	1,08	507			1,91	
	42. 4	1,05	1,02	507			1,71	
	50.31	1,00	0,98	508			1,57	
	58.59	0,96	0,94	508			1,45	
	1. 7.27	0,93	0,90	508			1,32	
	15.55	0,88	0,86	508			1,21	
	24.23	0,84	0,82	508			1,10	
	32.51	0,81	0,79	508			1,02	
61,4	41.19	0,77						
61,3	Mean			507,6	505,6	86050,28	1,57	86051,85

in the length of the pendulum vibrating seconds.

497

May 13, A. M. SHANKLIN FARM.

Clock losing 9^s.4.

Barometer 30.08 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
60.8	h. m. s.	°	°				s.	
	9.12.17	1.16	1.14	506			2.13	
	20.43	1.12	1.09	507			1.95	
	29.10	1.06	1.04	507			1.77	
	37.37	1.02	0.99	508			1.60	
	46. 5	0.97	0.95	507			1.48	
	54.32	0.93	0.90	508			1.32	
	10. 3. 0	0.88	0.86	508			1.21	
	11.28	0.84	0.83	508			1.13	
	19.56	0.82	0.79	508			1.02	
	28.24	0.77	0.75	509			0.92	
60.9	36.53	0.73						
60.8	Mean			507.6	505.6	86050.28	1.45	86051.73

May 13, P. M.

Barometer 30.08 inches.

60.9	0.20.36	1.18	1.15	506			2.17	
	29. 2	1.13	1.10	507			1.99	
	37.29	1.08	1.05	507			1.81	
	45.56	1.03	1.00	507			1.64	
	54.23	0.98	0.96	507			1.51	
	1. 2.50	0.94	0.92	508			1.39	
	11.18	0.91	0.88	508			1.27	
	19.46	0.86	0.84	508			1.16	
	28.14	0.82	0.80	508			1.05	
	36.42	0.79	0.77	508			0.97	
61.1	45.10	0.76						
61.0	Mean			507.4	505.4	86050.14	1.50	86051.64

498 Capt. KATER's experiments for determining the variation

May 14, A. M. SHANKLIN FARM.

Clock losing 9', 4.

Barometer 30,14 inches.

Temp	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
60,4	h. m. s.	°	°				s.	
	9.35.41	1,19	1,16	507			2,20	
	44. 8	1,14	1,11	507			2,01	
	52.55	1,08	1,05	508			1,81	
	10. 1. 3	1,03	1,01	508			1,67	
	9.51	0,99	0,97	508			1,54	
	17.59	0,95	0,93	509			1,42	
	26.28	0,92	0,90	508			1,33	
	34.56	0,88	0,86	509			1,21	
	43.25	0,84	0,82	509			1,10	
	51.54	0,80	0,78	509			1,00	
60,7	11. 0.23	0,76						
60,5	Mean			508,2	506,2	86050,61	1,53	86052,14

May 14, P. M.

Barometer 30,10 inches.

60,7	0.16.14	1,18	1,15	507			2,17	
	24.41	1,15	1,10	507			1,99	
	33. 8	1,08	1,05	508			1,81	
	41.36	1,03	1,00	508			1,64	
	50. 4	0,98	0,96	508			1,51	
	58.32	0,94	0,92	508			1,39	
	1. 7. 0	0,90	0,88	508			1,27	
	15.28	0,86	0,84	508			1,16	
	23.56	0,82	0,80	509			1,05	
	32.25	0,78	0,76	509			0,95	
60,9	40.54	0,75						
60,8	Mean			508	506	86050,48	1,49	86051,97

May 15, A. M. SHANKLIN FARM.

Clock losing 9^s.4.

Barometer 30.05 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
60.5	h. m. s.	°	°				s.	
	9.18.29	1.18	1.15	506			2.17	
	26.55	1.12	1.09	507			1.95	
	35.22	1.07	1.04	507			1.77	
	43.49	1.02	0.99	507			1.60	
	52.16	0.97	0.95	506			1.48	
	10. 0.42	0.93	0.91	507			1.36	
	9. 9	0.90	0.88	508			1.27	
	17.37	0.87	0.85	508			1.29	
	26. 5	0.83	0.81	508			1.18	
	34.33	0.79	0.77	508			0.97	
61.4	43. 1	0.76						
60.9	Mean			507.2	505.2	86049.94	1.50	86051.44

May 15, P. M.

Barometer 30.05 inches.

61.3	0.31.38	1.17	1.14	506			2.13	
	40. 4	1.12	1.09	507			1.95	
	48.31	1.07	1.04	507			1.78	
	56.58	1.02	1.00	506			1.64	
	1. 5.24	0.98	0.95	507			1.48	
	13.51	0.93	0.91	507			1.36	
	22.18	0.89	0.87	508			1.24	
	31.46	0.85	0.83	507			1.13	
	39.13	0.82	0.80	507			1.05	
	47.40	0.78	0.76	508			0.95	
61.4	56. 8	0.75						
61.3	Mean			507	505	86049.81	1.47	86051.28

500 Capt. KATER's experiments for determining the variation

May 16, A. M. SHANKLIN FARM.

Clock losing 9^s.4.

Barometer 30.03 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	No. of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
59.8	h. m. s.	°	"				s.	
	9.29.58	1.20	1.17	507			2.24	
	38.25	1.14	1.11	506			2.02	
	46.51	1.09	1.06	507			1.81	
	55.18	1.04	1.02	507			1.71	
	10. 3.45	1.00	0.98	508			1.57	
	12.13	0.96	0.94	507			1.45	
	20.40	0.92	0.89	508			1.30	
	29. 8	0.87	0.85	508			1.20	
	37.36	0.83	0.81	509			1.18	
	46. 5	0.80	0.78	507			1.00	
60.4	54.32	0.76						
60.1	Mean			507.4	505.4	86050.14	1.56	86051.70

May 16, P. M.

Barometer 30.03 inches.

60.0	0.24.40	1.19	1.16	506			2.20	
	33. 8	1.13	1.10	506			1.99	
	41.32	1.08	1.05	507			1.81	
	49.59	1.03	1.00	507			1.64	
	58.26	0.98	0.96	507			1.51	
	1. 6.53	0.94	0.92	507			1.39	
	15.20	0.91	0.89	507			1.30	
	23.47	0.87	0.85	507			1.20	
	32.14	0.83	0.81	508			1.18	
	40.42	0.79	0.77	508			0.97	
60.8	49.10	0.76						
60.7	Mean			507	505	86049.81	1.53	86051.34

Observations for connecting the Stations of the Trigonometrical Survey with those of the Pendulum.

Clifton.

Oct. 9th 1818. The angles of the following triangles were observed, in order to obtain the distance from Clifton Beacon to the Pendulum.

Clifton Beacon from Laughton Spire, 254.09 feet.

Clifton Beacon,	$83.22.23''$	} to Station A {	934 feet.
Laughton Spire	$(2.6.0)$		
Station A,	-		94.31.37

Clifton Beacon from Station A, 934 feet.

Clifton Beacon,	$85.48.29''$	} to Pendulum Stat. {	1380 feet.
Station A,	-		
Pendulum Station,	$(35.22.50)$		

The angle between Laughton Spire and the Pendulum Station is, - - } $169.10.52''$

Laughton Spire is south west of the Meridian of Clifton Beacon, - - } 156.12

Hence, the bearing of the Pendulum Station from Clifton Beacon to the N.E. is } $12.45.20$

Arbury Hill.

On the 26th of October, a base of 906 feet was measured in the meadows at the foot of Arbury Hill, for the purpose of finding the distance from Arbury Hill to the Pendulum

502 *Capt. KATER's experiments for determining the variation*

Station. As the house could not be seen, I chose a station (B) near it, which by measurement was 206 feet to the north of the clock. The following triangles were then observed.

From the North end of the Base to the South end, 906 feet.

North end,	-	$97^{\circ} 37' 5''$	} to Arbury Hill { $\overline{\hspace{1cm}}$
South end,	-	$54.30.21$	
Arbury Hill,	-	$(27.52.34)$	

From the South end of the Base to Arbury Hill, 1921 feet.

South end,	-	$104^{\circ} 24' 17''$	} to Station B. { $\overline{\hspace{1cm}}$
Arbury Hill,	-	$(34.42.4)$	
Station B,	-	$40.53.39$	

Adding 206 feet to 2842, we obtain 3048 feet, for the distance from the Pendulum to Arbury Hill, which was so nearly in the direction of the meridian as to require no correction.

Dunnose.

9th May, 1819, measured a Base of 1140 feet on Shanklin Down, and observed the following readings on the azimuth circle.

<i>At the North end of the Base.</i>		
Objects.	Readings of the Verniers.	Mean.
Summer house chimney, - -	$\begin{array}{r} 0.42.15 \\ 42.50 \\ 42.50 \end{array} \left. \vphantom{\begin{array}{r} 0.42.15 \\ 42.50 \\ 42.50 \end{array}} \right\}$	$\begin{array}{r} 0 \quad / \quad '' \\ 0.42.38 \end{array}$
South end of base, - - -	$\begin{array}{r} 106.11.20 \\ 11. \quad 5 \\ 11. \quad 5 \end{array} \left. \vphantom{\begin{array}{r} 106.11.20 \\ 11. \quad 5 \\ 11. \quad 5 \end{array}} \right\}$	106.11.10
Top of the Signal Post, - -	$\begin{array}{r} 210.15.40 \\ 16.20 \\ 15.35 \end{array} \left. \vphantom{\begin{array}{r} 210.15.40 \\ 16.20 \\ 15.35 \end{array}} \right\}$	210.15.52
Dunnose Station, - - -	$\begin{array}{r} 235.50.10 \\ 51. \quad 0 \\ 50.30 \end{array} \left. \vphantom{\begin{array}{r} 235.50.10 \\ 51. \quad 0 \\ 50.30 \end{array}} \right\}$	235.50.33
<i>At the South end of the Base.</i>		
Sir RICHARD WORSLEY's Obelisk,	$\begin{array}{r} 0.25.40 \\ 25.35 \\ 25.35 \end{array} \left. \vphantom{\begin{array}{r} 0.25.40 \\ 25.35 \\ 25.35 \end{array}} \right\}$	0.25.37
Dunnose Station, - - -	$\begin{array}{r} 57.19.55 \\ 19.50 \\ 19.45 \end{array} \left. \vphantom{\begin{array}{r} 57.19.55 \\ 19.50 \\ 19.45 \end{array}} \right\}$	57.19.50
Top of the Signal Post, - -	$\begin{array}{r} 57.56.20 \\ 56. \quad 5 \\ 56. \quad 0 \end{array} \left. \vphantom{\begin{array}{r} 57.56.20 \\ 56. \quad 5 \\ 56. \quad 0 \end{array}} \right\}$	57.56. 8
North end of base, - - -	$\begin{array}{r} 65.41. \quad 0 \\ 40.40 \\ 40.35 \end{array} \left. \vphantom{\begin{array}{r} 65.41. \quad 0 \\ 40.40 \\ 40.35 \end{array}} \right\}$	65.40.45
Summer House chimney, - -	$\begin{array}{r} 125. \quad 3.25 \\ 3.25 \\ 3.25 \end{array} \left. \vphantom{\begin{array}{r} 125. \quad 3.25 \\ 3.25 \\ 3.25 \end{array}} \right\}$	125. 3.25

At Dumose Station.

Object.	Readings of the Vertical.	Mean.
North end of Base,	0.17. 0 16.40 16.50	0.16.50
Top of the Signal Post,	36.26.20 26.20 26.15	36.26.18
Sir RICHARD WORSLEY'S Obelisk,	159.26.20 26.30 26. 0	159.26.17

No. 1. From the North to South end of Base, 1140 feet.

North end Base,	-	105.28.32	} to Summer house {	3753 chimney, { 4205
South end Base,	-	59.22.40		
Summer house,		(15. 8.18)		

No. 2. From the North to South end of Base, 1140 feet.

North end Base,	104. 4.42	} to Signal Post {	165 1191
South end Base,	7.44.37		
Signal Post,	- (68.10.41)		

No. 3. In the following triangle, we have given the two sides from the south end of the Base to the Summer house, and from the south end of the Base to the Signal Post and the included angle, to find the remaining angles and the distance from the Signal Post to the Summer house.

South end Base,	67. 7.17	} to Signal Post {	1191 8900
Summer house,	16.20.38		
Signal Post,	- 96.32. 5		

The distance from the Signal Post to the gun marking Dunnose station, was found by measurement to be 120 feet, the gun being to the northward, and nearly in a right line with the south end of the base and the Signal Post. This being added to 1191 feet, the distance of the Signal Post from the south end of the Base, we have 1311 feet, for the distance of the gun from the south end of the Base. In the following triangle therefore, two sides, and the included angle, are given to find the remaining angles and the third side.

No. 4. *From the South end of Base to Dunnose Station 1311 feet.*

South end Base,	$67^{\circ}43'35''$	} to Summer house	} <u>3901</u>
Dunnose Station,	$(94^{\circ}9'24'')$		
Summer house,	$(18^{\circ}7'1'')$		

The following angles are for the purpose of determining the angle at Dunnose station, between the north and south ends of the base.

North end Base,	-	-	-	$129^{\circ}39'23''$
South end Base,	-	-	-	$8^{\circ}20'55''$
Dunnose Station,	-	-	-	$(41^{\circ}59'42'')$

In the triangle No. 4, if from $94^{\circ}9'24''$ we subtract $41^{\circ}59'42''$ the remainder $52^{\circ}9'42''$ will be the angle at Dunnose Station, between the Summer house and the north end of the Base; to which the observed angle between the north end of the Base and Sir RICHARD WORSLEY'S Obelisk $159^{\circ}9'27''$ being added, we obtain $211^{\circ}19'9''$, or $148^{\circ}40'51''$ for the

in the length of the pendulum vibrating seconds. 507

By the Trigonometrical Survey, Dunnose appears to be 792 feet above the level of the sea; the height therefore of the Pendulum above the sea was 253 feet.

Observations with a Barometer of Sir H. ENGLEFIELD'S construction at the Isle of Wight.

Date.	Thermometer.	Stations.	Barometer. inches.	Calculated height, and correction	Feet above high water mark.
May 12	62	Summer house, - -	30,078	217,2 +7,0	224,2
	61	High water mark, - -	30,314		
	63	Summer house, - -	30,092		
15	62	Summer house, - -	30,036	209,8 +6,7	216,5
	61	High water mark, - -	30,260		
16	61	Summer house, - -	30,015	212,8 +7,0 +2,0	221,8
	56	Beach (2 ft. above h. water,) - -	30,227		
	61	Summer house, - -	30,008		
				Mean	220,8
15	60	Dunnose, - -	29,499	707,7 +22,8	730,5
	61	High water mark, - -	30,260		
	60	Dunnose, - -	29,499	497,4 +16,1	513,4
	62	Summer house, - -	30,036		

The correction applied, is $\frac{1}{36}$ of the calculated height on account of the rise of the mercury in the cistern of the barometer.

From the preceding table we have		
Dunnose above the Summer house by Trigonometrical measurement,	- - - - -	589,0
By the Barometer,	- - - - -	513,4
Difference,	-	25,6

Summer house above high water mark by the baro-	}	220,8
meter, - - - - -		
Add for the fall of the tide, - - -		10,0
		<hr/>
Summer house above low water, - - -		230,8
Above low water by the Survey and Trigonometri-	}	253,0
cal observation, - - - - -		
		<hr/>
Difference -		22,2

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March 4. Annals of Philosophy, No. 75.

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- The European Magazine for April.
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Berlin, 1818. 4°

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The Philosophical Magazine, No. 253.

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The Monthly Review from June.

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INDEX

TO THE

PHILOSOPHICAL TRANSACTIONS

FOR THE YEAR 1819.

A	page
<i>Amidine</i> , matière intermédiaire entre la gomme et l'amidon,	52
<i>Amidon</i> , observations sur sa décomposition à la température atmosphérique par l'action de l'air et de l'eau -	29
— sur la gomme produite par sa fermentation spontanée, -	50
— sur le changement de poids qu'il éprouve par sa fermentation à l'air, -	54
— sur le gaz hydrogène produit par sa fermentation, -	57
ANDERSON, JAMES, Capt. in the Royal Navy, some observations on the peculiarity of the tides between Fairleigh and the North Foreland; with an explanation of the supposed meeting of the tides near Dungeness, -	217
<i>Arbury Hill</i> , operations for determining the variation in the length of the pendulum vibrating seconds at that station in the Trigonometrical Survey, -	374
— its latitude, -	403
B	
BABBAGE, CHARLES, Esq., on some new methods of investigating the sums of several classes of infinite series,	249
BREWSTER, DAVID, LL. D., on the laws which regulate the absorption of polarised light by doubly refracting crystals, -	11

INDEX.

BREWSTER, DAVID, LL. D., on the action of crystallized surfaces upon light,	page 145
on the optical and physical properties of Tabasheer,	283
BRINKLEY, JOHN, D. D., the results of observations made at the Observatory of Trinity College, Dublin, for determining the obliquity of the ecliptic, and the maximum of the aberration of light,	241

C

<i>Chronometer</i> , observations for the error of it,	439
<i>Clifton, in Yorkshire</i> , operations for determining the variation in the length of the pendulum vibrating seconds at that station in the Trigonometrical Survey,	371
its latitude,	395
<i>Clock</i> , comparisons of it with the chronometer,	434
observations for determining its rate at different stations of the Trigonometrical Survey,	427
<i>Corpora lutea</i> , a treatise on the subject of them	59
are produced previous to, and independent of, sexual intercourse,	60
never met with before puberty,	59
<i>Crystals, absorbing</i> , list of those with one axis,	15
list of those with two axes,	19
<i>Crystals, doubly refracting</i> , on the influence of their polarising force, upon the polarising force which accompanies partial reflection,	150
on the change produced upon the polarising angle by their interior forces,	150
on the change produced upon the polarisation of the reflected ray by their interior forces,	152

D

<i>Davis's Straits, and Baffin's Bay</i> , observations to determine the variation of the needle in those places,	140
DAVY, SIR HUMPHRY, BART., some observations on the formation of mists in particular situations,	123
<i>Dungeness</i> , meeting of the tides there,	223
<i>Dunmose</i> , operations for determining the variation in the length of the pendulum vibrating seconds at that station in the Trigonometrical Survey,	379

INDEX.

page

E

<i>Earth</i> , on its mean density,	-	-	-	84
—— on the irregularities of its surface,	-	-	-	89
—— of its figure,	-	-	-	417
<i>Ecliptic</i> , results of observations made at the Observatory of Trinity College, Dublin, for determining the obliquity of it, and the maximum of the aberration of light,	-	-	-	241
<i>Equations, numerical</i> , a new method of solving all orders of them by continuous approximation,	-	-	-	308
<i>Error</i> , remarks on the probabilities of it in physical observations, and on the density of the earth, considered especially with regard to the reduction of experiments on the pendulum,	-	-	-	70
<i>Eye</i> , an account of a membrane in the eye, now first described,	-	-	-	300

<i>Fairleigh and the North Foreland</i> , phenomena of the tides between them on the English coast, and Cape d'Alprée and Calais on the French coast,	-	-	-	218
---	---	---	---	-----

H

<i>Heat</i> , on the influence it possesses in modifying the absorbent power of crystals,	-	-	-	25
HOME, SIR EVERARD, BART., the Croonian Lecture. On the conversion of pus into granulations of new flesh,	-	-	-	1
—— on corpora lutea,	-	-	-	59
—— an account of the fossil skeleton of the Proteosaurus,	-	-	-	209
—— reasons for giving the name Proteosaurus to the fossil skeleton of an animal, which has been described,	-	-	-	212
—— on the ova of the different tribes of opossum and ornithorhyncus,	-	-	-	234
HORNER, W. G. ESQ., a new method of solving numerical equations of all orders, by continuous approximation,	-	-	-	308

INDEX.

page

JACOB, ARTHUR, M. D. an account of a membrane in the eye, now first described,	-	-	-	300
<i>Isle of Wight</i> , operations for determining the variation in the length of the pendulum vibrating seconds at that station in the Trigonometrical Survey,	-	-	-	379

K

<i>Kangaroo</i> , the ova of that animal described,	-	235, 237
KATER, CAPT. HENRY. An account of experiments for determining the variation in the length of the pendulum vibrating seconds at the principal stations of the Trigonometrical Survey of Great Britain,	-	337
— Appendix to his Account of experiments, &c.	-	427

<i>Latitudes and Longitudes</i> of the different stations in the Trigonometrical Survey,	-	-	-	384
<i>Leith Fort</i> , operations for determining the variation in the length of the pendulum vibrating seconds at that station of the Trigonometrical Survey,	-	-	-	364
— its latitude,	-	-	-	392
<i>Light</i> , on the action of crystallized surfaces upon it,	-	-	-	145
— <i>polarised</i> , on the laws which regulate its absorption by doubly refracting crystals,	-	-	-	11
— on the absorption of it by crystals with one axis of double refraction,	-	-	-	12
on the absorption of it by crystals with two axes of double refraction,	-	-	-	17
<i>transmitted</i> , on the effects produced upon it by a change in the mechanical condition of the surfaces of crystals,	-	-	-	146
<i>London</i> , operations for determining the variation in the length of the pendulum vibrating seconds at that station in the Trigonometrical Survey,	-	-	-	377
— its latitude,	-	-	-	407

INDEX.

page

M

MARCET, ALEXANDER, M. D. on the specific gravity and temperature of sea waters, in different parts of the ocean, and in particular seas; with some account of their saline contents,	- - -	161
<i>Mists</i> , some observations on their formation in particular situations,	- - -	123

N

<i>Needle, magnetic</i> , on the anomaly in the variation of it as observed on ship-board,	- - -	96
————— observations on the dip and variation of it, and on the intensity of the magnetic force; made during the late voyage in search of a north-west passage,	- - -	132
<i>Needles, compass</i> , on the irregularities observed in the direction of those of H. M. S. <i>Isabella</i> and <i>Alexander</i> in their late voyage of discovery, and caused by the attraction of the iron contained in the ships,	- - -	112

O

<i>Observations, multiplied</i> , on the estimation of the advantage to be derived from them,	- - -	70
<i>Ocythoë</i> , on that genus,	- - -	107
<i>Opossum</i> , on the ova of that tribe,	- - -	234
<i>Ornithorhyncus</i> , on the ova of that tribe,	- - -	234
<i>Ovarium, human</i> , its natural structure described,	- - -	59
<i>Pendulum, rolling</i> , Euler's formula for it,	- - -	94
<i>Pendulum vibrating seconds</i> , an account of experiments for determining the variation in its length, at the principal stations of the Trigonometrical Survey of Great Britain,	- - -	337
————— description of the one used in the above experiments,	- - -	341
the mode of determining its expansion, and a statement of the results,	- - -	343

INDEX

	<i>page</i>
<i>Portsoy</i> , operations for determining the variation in the length of the pendulum vibrating seconds at that station of the Trigonometrical Survey, - - -	356
----- its latitude, - - -	389
<i>Presents</i> , a list of those made to the Royal Society, from November 1818, to July 1819, - - -	509
<i>Proteosaurus</i> , an account of the fossil skeleton of that animal, - - -	209
----- reasons for giving this name to the fossil skeleton which has been described, - - -	212
<i>Pus</i> , on the conversion of it into granulations of new flesh, - - -	1
----- changes which it undergoes upon the surface of a sore, - - -	3
----- its presence necessary to produce granulations or new flesh in sores, - - -	7
----- <i>coagulated</i> , rendered tubular by the extrication of its carbonic acid gas, - - -	9

R

<i>Refraction</i> , corrections for it, - - -	160*
---	------

S

SABINE , CAPT. EDWARD, on irregularities observed in the direction of the compass needles of H. M. S. <i>Isabella</i> and <i>Alexander</i> , in their late voyage of discovery, and caused by the attraction of the iron contained in the ships, - - -	112
----- observations on the dip and variation of the magnetic needle, and on the intensity of the magnetic force; made during the late voyage in search of a north-west passage, - - -	132
<i>Sal-ammoniac</i> , a saturated solution of it has a greater power of coagulating pus than any other substance, - - -	7
----- its effects upon the surface of a sore, - - -	8
SAUSSURE , THEODORE DE, (Professeur de Minéralogie dans l'Académie de Genève, &c.) observations sur la décomposition de l'amidon à la température atmosphérique par l'action de l'air et de l'eau, - - -	29
MDCCOCXIX. 8 Y	

INDEX.

	<i>page</i>
SAY, THOMAS, ESQ. on the genus <i>ocythoë</i> , -	107
SCORESBY, WILLIAM, JUN. ESQ., on the anomaly in the variation of the magnetic needle, as observed on ship-board, - - -	96
<i>Sea water</i> , its temperature at the surface, and at different depths, as observed by Lieut. PARRY and others,	203
<i>Sea waters</i> , on their specific gravity and temperature, in different parts of the ocean, and in particular seas; with some account of their saline contents, - - -	161
----- their specific gravities, - - -	169
----- on their saline contents, - - -	191
<i>Series, infinite</i> , on some new methods of investigating the sums of several classes of them, - - -	249
<i>Serum</i> , experiments made by Mr. BAUER, to prove that globules are produced in it, - - -	2
<i>Shanklin Farm, in the Isle of Wight</i> , its latitude, -	408
 <i>Tabasheer</i> , on its optical and physical properties,	 283
----- the absolute refractive power of that and other bodies, - - -	287
<i>Tides</i> , some observations on the peculiarity of those between Fairleigh and the North Foreland; with an explanation of the supposed meeting of the tides near Dungeness, - - -	217
<i>Trigonometrical Survey of Great Britain</i> , observations for connecting the stations of it with those of the pendulum, - - -	501

U

<i>Unst</i> , operations for determining the variation in the length of the pendulum vibrating seconds at that station of the Trigonometrical Survey, - - -	344
---	-----

W

<i>Water</i> , a description of an apparatus for raising it from the bottom of the sea,	164
---	-----

INDEX.

page

Y

YOUNG, THOMAS, M. D. remarks on the probabilities of error in physical observations, and on the density of the earth, considered especially with regard to the reduction of experiments on the pendulum,	-	70
Postscript to his remarks on the reduction of experiments, &c.	-	160*

ERRATA.

P.	L.
32,	10, for 100 : 180.46, read, 100 : 80.46.
35,	12, note, for ce dernier aussi, read, ce dernier ainsi.
35,	13, note, for une légère, read, une légère.
36,	18, for à $\frac{1}{2}$, read, à $\frac{1}{2}$ éme.
39,	14, for rapport de 100.83, read, rapport de 100 : 83.
41,	6, for ligneux amilacé 92, read, ligneux amilacé 9,2.
42,	6, for 26 centim. cubes, read, 96 centim. cubes.
45,	6, for 42 heures, read, 42 jours.
196,	14, for 374, read 3.74.
—	20, for 0.314, read 39.23.
—	21, for be be, read be.
199,	8, and 9, for so that, read and that.
—	19, for 0.975, read 0.995.
—	21, for 21.460, read 21.580.

